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JOURNAL OF THE ROYAL STATISTICAL SOCIETY

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Vol. 121.] [Part I

Journal of the Royal Statistical Society

SERIES A (GENERAL)

PART I, 1958.

Recent Developments in Economic Statistics

[The Inaugural Address of the President, Sir Harry Campion, C.B., C.B.E., delivered to the Royal Statistical Society on November 20th, 1957.]

I must begin by thanking Fellows for electing me as President of the Society. I appreciate the honour, the more especially since it was through the activities of the Society that as a young post-graduate student I first met Stamp, Flux and Bowley, with whom I had the opportunity of working later. They made me very aware of the traditions of the Society and the place it holds in the development of statistics in this country and abroad.

When I joined the Society, these three were in their heyday and I do not think even they realized the developments which were to take place in statistical methods and in the widening range of applications. When I joined, the Society had 1,000 Fellows and had meetings only in London. Now it has 2,800 Fellows, four Sections organizing meetings on different branches of statistics, nine local groups outside London, and it publishes three journals.

The widening range of applications for statistical methods has led inevitably to a greater specialization of interests between members of the Society, if only because to do their best work statisticians must be familiar with the nuances of the data they use, and in consequence they become identified with a particular field of application. On the other hand the all-round development in recent years of statistical methods and of techniques of analysis, which can be usefully employed in many fields, has provided a strong unifying influence. Personally I have always thought the meetings of the Society serve a most valuable purpose in bringing and keeping together those concerned in the improvement of statistical methods and those primarily engaged in the practical applications of statistical techniques—the tool makers and the tool users.

But the Society also includes among its Fellows many who are not themselves engaged in the practical application of statistical techniques but in using statistics prepared by others, and outside the Society there is a large and increasing number of people who are users and not compilers of statistics. To secure and maintain close liaison between those who prepare and those who use statistics is one of the aims of our Society, and I have a feeling that the need to do so is more important in the case of economic and social statistics than perhaps in other branches of statistics where the principal users are more professional in their training and scientific in their outlook. In the nineteenth century the meetings of our Society brought together the producers and users of economic statistics. The establishment of the

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Royal Economic Society in 1890 as a separate body was the first break in this liaison, and although the Royal Statistical Society and the Royal Economic Society still have the same address, in Bentinck Street, London, I feel something has gradually been lost which is difficult to recapture. It is this which explains my choice of subject for this Address.

The progress made in economic statistics since before the war has been so great and the uses to which statistics are now put so influential in the formulation and direction of economic and financial policy, that I thought I might take this opportunity to say something of recent developments, of the ways it has already been possible to extend and improve economic statistics and what it is hoped to do, More particularly, I should like to speak of the general and technical problems which have been encountered—some solved and others still unsolved—and of the increased responsibility which the economic statistician is now expected to bear. Much misunderstanding can be avoided if these problems are known and understood not only by the statisticians themselves but more so by the users of statistics.

1

Let me start by going back to the position before the war. Without wishing to belittle the efforts of our predecessors, the development of economic statistics in this country in the years before the war appears to present eyes to have been slow and uneven. The collection of statistics on a national scale was brought about by the necessity to know the facts about some urgent economic and social problems or as a by-product of the working of administration rather than as part of any comprehensive programme on the part of the Government for enlarging its knowledge of the resources of the country and the condition of the people. Private investigations often preceded the development of official statistics in different fields of enquiry, and the Journals of the Society are full of accounts of ambitious statistical enquiries undertaken by private persons, often working alone.

It was not possible to do much during the war years in thinking in detail about long-term improvements in economic statistics. The statistical activities of Government Departments increased rapidly but were primarily concerned with preparing the kind of statistics needed for the direction of the war effort. However, some beneficial legacies did come from war experience. The first and, I think, the most important, was that statisticians were able to show in practice how much statistics could be used effectively in day-to-day administrative problems on a wide range of subjects, and the statisticians of Departments got the habit of working together. The second was that some kinds of statistics were developed which it was later realized would be most useful in peace time. Examples of these were more comprehensive monthly and quarterly figures of employment collected from firms, monthly figures of output, consumption and stocks of a large range of raw materials and manufactured goods, and, perhaps of more significance, estimates of national income and expenditure which had never previously been prepared officially.

The first real opportunity to look at the possible long-term development of economic statistics came when the Government accepted as one of their primary aims after the war the maintenance of a high level of employment and issued in May, 1944 the White Paper on Employment Policy (Cmd. 6527). In preparing this White Paper the kinds of statistics needed to carry out this policy in practice were considered in detail and a summary of them appeared in the 1944 White Paper. It is worth while recalling what these were since the paragraphs in the White Paper became the gospel for the development of economic statistics afterwards.

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83. The following are the principal classes of statistics (in addition to those available before the war) which must be obtained for the efficient operation of an employment policy:—

(*a*) Statistics of employment and unemployment, including quarterly or monthly statements of present and prospective employment in the main industries and areas in the country, based on returns from employers.

(*b*) Regular information relating to savings, projected capital expenditure by public authorities, and, as far as possible, by private industry.

(*c*) An annual census of production showing the structure of the main groups of industries in the preceding year, including, *inter alia*, details of the quantity and value of output, stocks, and work in progress.

(*d*) Monthly figures of production, consumption and stocks, and, if possible, figures of orders on hand, based on sample returns obtained periodically throughout the year from large firms, trade associations, and public institutions.

(*e*) Annual and quarterly estimates of foreign capital movements and balance of foreign payments.

84. It is also proposed to develop the annual White Paper on National Income and Expenditure by providing a much more complete analysis than has hitherto been possible of the constituent parts of the country's total expenditure. In particular, direct estimates will be made of the various types of capital expenditure and the various sources of savings. This will be, in effect, the Capital Budget of the nation's wealth.

These were not written as pious hopes but were intended as a basis for action. An operational report was prepared setting out in detail the various kinds of statistics which might be needed and also the allocation of responsibilities which Departments might be expected to assume, and the legal powers which would be involved in collecting the information. It was on the basis of the paragraphs in the 1944 White Paper, and supporting documents that steps were taken to get approval for the Statistics of Trade Act, 1947—the first general Act governing the collection of statistics in this country. The first clause of this Act starts with the words "For the purpose of obtaining the information necessary for the appreciation of economic trends . . . ".

The publication of these proposals in the White Paper was evidence of a marked change in the attitude towards the collection and use of statistics compared with before the war. It was realized that economic statistics should be developed according to a long-term comprehensive programme; more attention should be paid to getting the best out of the data collected by using similar classifications and time periods for series to be compared, and, if possible, the data should fit into a common framework. The tables introduced during the war on national income and expenditure and the sector accounts provided a possible basis for such a framework. Something much more was wanted however, than devising model schemes. No longer could Government Departments wait for the pioneering efforts of private investigators. The Statistics Divisions of Departments must take the initiative to collect the data needed, using the most advanced statistical methods available and to a great measure also doing their own research to find the most suitable methods of collection and analyses. Most of the data henceforward must be collected by the statisti-

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cians themselves by direct statistical enquiries and not obtained as by-products of the working of administration.

The broad framework used for the development of short period statistics is given in Table A. It followed the system of national income accounts in trying to measure changes in—

(*a*) national expenditure and the balance of payments;
(*b*) national product and the supply of resources;
(*c*) the income generated in producing the national product, and also the changes taking place in—
(*d*) employment and unemployment;
(*e*) prices, and
(*f*) money supply and finance.

The idea was to prepare annual estimates of the principal aggregates shown in the first column of the Table, and for shorter periods to have a running set of series indicative of changes in these aggregates and a set of forward looking indicators so far as it was possible to obtain these.

During the last ten years statisticians in Government Departments, with the help they have so willingly been given from statisticians in industry and trade, have been tackling the job of building up statistics in these new fields and improving the coverage and frequency of existing series, for example, of employment, industrial production, capital formation, stocks, retail trade, balance of payments, incomes, and prices.

As these new and improved statistical series have come into use, and have been found to be valuable, there have been further demands to get the information more quickly, and in some instances to make the statistics more comprehensive. In the House of Commons on August 1st, 1956, the Chancellor of the Exchequer (Mr. Macmillan) made a Statement on new developments he thought necessary. He said that further improvements should be made in the figures of (*a*) fixed investment and stocks, particularly in the distributive and service trades, (*b*) investment in building, (*c*) profits, (*d*) balance of payments, especially capital transactions, and mentioned (*e*) that there would be a continuing family budget enquiry to provide up-to-date information of changes in income and expenditure. He stressed that "our first need is to be up to date".

This desire for more up-to-date figures has another side to it. A broad analysis of the economic situation, as given for example in the annual Economic Surveys, is now made usually in terms of the main aggregates used in national income analysis, for example, changes in personal consumption, capital investment and the like. It would be equally desirable to present economic reviews for periods shorter than a year in the same terms and so avoid having to switch from using one set of economic indicators for annual reviews and a different set of indicators for shorter periods. One result of getting more up-to-date and comprehensive figures is that it is now possible to prepare quarterly estimates of some of the main items of national income and expenditure.

II

I could not hope to give in this Address a detailed account of all the new statistics which have been produced in the last ten years. In any case explanatory notes on many of these have already appeared in official publications and in the Journals of the Society. There

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TABLE A.—*Model Framework for the Development of Short Period Statistics for Current Economic Analysis*

Economic Subjects to be Studied	*Current Indicators*	*Forward-looking Indicators*
A. NATIONAL EXPENDITURE 1. *Consumers' expenditure: analysis by type of commodity or service, Distinguishing—* (*a*) durable goods (*b*) non-durable goods (*c*) services	(i) statistics of retail sales (ii) family budget enquiries (iii) statistics of production and supply of individual commodities (adjusted for imports and exports, and, if possible, stock changes)	(i) surveys of consumers' intentions.
2. *Gross fixed capital formation—* (*a*) houses (*b*) other building and construction by— (i) enterprises (private) (ii) public authorities (*c*) plant and machinery by— (i) enterprises (private) (ii) public authorities	(i) statistics of output of the construction industries (ii) statistics of output of plant and machinery (adjusted for imports and exports) (iii) expenditure by firms on capital account	(i) surveys of investment intentions of private enterprises. (ii) investment plans or programmes for the public sector. (iii) statistics of permissions to build, contracts placed, construction started. (iv) new and unfilled orders for capital goods of engineering and construction firms.
3. *Stocks—* (*a*) agriculture (*b*) mining, manufacturing, construction, etc. (i) raw materials (ii) work in progress (iii) finished products (*c*) wholesale trade (*d*) retail trade (*e*) Central Government	(i) statistics of the value of stocks collected from enterprises (ii) estimates derived from statistics of sales, and purchases and of production and consumption of enterprises (iii) statistics on stocks of individual commodities	(i) statistics of applications for credit.
4. *Public Authorities' current expenditure*	(i) monthly and quarterly financial statements (ii) special statistical tabulations of government expenditure	(i) financial budgets of the Central Government and other public authorities.
5. *Balance of payments*	(i) statistics of overseas trade and other international transactions	(i) for particular items: export orders and import commitments.
B. NATIONAL PRODUCT 1. *agriculture* 2. *mining, manufacturing and public utilities* 3. *construction industries* 4. *wholesale trade* 5. *retail trade* 6. *transport* 7. *services*	(i) statistics of output of individual commodities (ii) other series indicative of change in output, for example, labour employed, deliveries, sales (iii) retail sales statistics (iv) sales by wholesalers (v) traffic receipts	(i) crop expectations, area sown, livestock numbers. (ii) new and unfilled orders. (iii) statistics of contracts placed, houses and buildings started.
C. INCOME 1. *income from employment* (*a*) total wages paid (*b*) wage rates (*c*) average weekly earnings 2. *income from agriculture and from work on own account* 3. *profits* 4. *profits and surpluses of Public Authorities*	(i) indices of wage rates and earnings enquiries (ii) P.A.Y.E. tax receipts (iii) company reports and quarterly samples of profits (iv) monthly and quarterly financial statements by Central Government	(i) financial budgets of the Central Government and Local Authorities.
D. EMPLOYMENT 1. *number employed* 2. *number unemployed* 3. *hours worked*	(i) returns from employers of numbers employed and of the numbers working short-time and overtime (ii) returns from labour exchanges of numbers unemployed	(i) statistics of unfilled vacancies and of vacancies notified.
E. PRICES 1. *retail prices* 2. *wholesale prices* 3. *import prices* 4. *export prices*	(i) data from retail shops and manufacturers and distributors (ii) analyses of import and export returns	
F. MONEY, SUPPLY AND FINANCE 1. *money supply* 2. (*a*) *savings by:* (i) Persons (ii) enterprises (iii) Central Government (*b*) *credits to:* (i) Persons (ii) enterprises (iii) Central Government	(i) returns from banks of deposits and advances (ii) currency circulation (iii) returns from banks and financial institutions (iv) analyses of company accounts (v) hire purchase statististics (vi) financial statements by Government Departments	

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are, however, some general considerations which the experience of trying to carry through this programme for the development of economic statistics has raised.

The first I would mention is that while an attempt has been made to fit the collection and analysis of data into a common systematic framework, in practice it has not been possible to fit all the desirable lines of development into such a framework. The needs for information on individual industries and trades and on particular aspects of the economic situation have to be judged on their own merits and it would be wrong to discourage improvements which may not primarily be made for getting better short-term indicators or more complete national income and expenditure estimates. What the use of a common framework has done has been to impress on those responsible for compiling individual statistical series that those series would be used not in isolation but together with other series to study the economic situation, so that they should be seeking improvements not only in precision of measurement of individual series—and this has become more necessary—but also comparability, so far as possible, in scope, definition, timing and base periods, with other series. Further, since to throw light on some dark corners in our knowledge of the economic situation is everybody's business but nobody's special concern, the use of a common framework for the development of economic statistics has made sure that these are not missed.

Secondly, it should be stressed that while the paragraphs on statistics in the 1944 White Paper on Employment Policy have been used as a gospel for the post-war development of economic statistics, they were not drafted to provide information to implement one kind of Government policy but to assess the needs and judge the successes of alternative lines of policy. (I remember Keynes saying that he thought these paragraphs on statistics some of the most important in the White Paper for, if the information could be obtained, a future Government would then be in a much better position to decide between different ideas on economic and financial policy, including his own.) The programme for the development of economic statistics was therefore deliberately drawn up with a broad and not a limited objective in view. On the other hand, the kinds of statistics to be collected must inevitably be influenced by changes in the direction of Government policy. The information available on output, stocks and consumption of materials and foodstuffs, and of capital investment in private industry, needed to be more detailed in some ways, when allocation and rationing schemes were still in operation after the war, than it is now, and conversely greater reliance on monetary controls in recent years has made it necessary to look again at the adequacy of financial and monetary statistics.

The third point I would emphasize is that it is not only the needs of the Government for statistics for its own use which must be borne in mind. There are the needs of firms in industry, trade and finance, of trade organizations, of market research agencies, of economists and financial writers, of research workers, and also of the general public. Their needs are more diffuse than those of the Government itself. What firms in industry and trade usually want are detailed figures about their own line of business to set off and compare with the economic indicators for the economy as a whole. Simple totals, for example, of the output of iron and steel, cotton yarn, or of other commodities are of limited use to firms in those trades, even though they might be enough for someone writing generally about the economic situation, or as single items in preparing a general index of industrial activity. It is not possible to streamline the collection of figures from industry and trade only to the broad totals which outside users of the statistics may think are needed. Those who supply the basic data must feel that the results are of use to them.

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Fourthly, statisticians should have a knowledge of what firms in industry and trade can provide most readily from their own internal records. Good personal relations between statisticians and firms supplying the figures have become even more important in recent years because in some of the newer enquiries, for example on capital expenditure, stocks and profits, questions are asked which some firms find difficult to answer, and further, even if they can provide the figures, they regard them of a much more confidential nature than other figures they have been previously asked to give—for example, of numbers employed. It has often been said that many economic statistics are forthcoming as a by-product of the working of administration and I myself quoted this remark earlier. This, as a general statement, is much less true than it was. Many of the new statistics being collected are obtained by direct statistical enquiries. The limits to the kind of statistics obtained and the definitions employed are more largely determined by the questions which small and large businesses can answer from the records they keep or can be persuaded to keep.

Many figures are collected from firms voluntarily without the use of compulsory legal powers. Even when compulsory powers are used, these powers either do not stipulate an exact period of time within which each class of figures must be sent in, or if they do, the time permitted for completing the returns is set tolerantly. To obtain up-to-date and accurate figures quickly, whether voluntarily or under compulsory powers, depends to a large extent on securing the goodwill of firms in industry and trade.

Fifthly, recent experience has more than confirmed the warning given ten years ago by the Clapham Committee (Cmd. 6868) when it reported that "now that the war is over, a chronic struggle is going on between the universities, business and the public service for the services of a supply of statisticians which is in the aggregate equal to only a tiny fraction of the demands immediately manifesting themselves, and still less of the demands which are certain to accrue as current policies develop and projected institutions are brought into being. There are few more urgent needs today than the increase of the supply of first-class statisticians". The Committee's view was "An adequate supply of statistical competence is quite fundamental to the advancement of knowledge of social and economic questions".

It was realised that the programme envisaged in the 1944 White Paper on Employment Policy for the development of economic statistics could only be carried through if enough trained statisticians could be recruited to do the job. In spite of the warning of the Clapham Committee, I do not think anyone realized how serious the shortage was likely to be, how long it would last, and that ten years after the end of the war the Universities should be still only turning out so few economic statisticians. There are no doubt a number of reasons for this, as Fellows will be aware. Students with a mathematical bent have been taking jobs in science and technology and not enough graduates in economics have been getting basic training in statistics. Whatever the reasons, there is no doubt that new developments in economic statistics have been handicapped by the lack of trained staff.

Moreover, it has not been easy to seek expert advice outside Government Departments. Senior University posts in economic statistics in this country are still relatively few. The number of chairs in economic statistics can still be counted on the fingers of one hand. It has been possible for valuable work, for example, on input/output studies and on saving to be done in research institutions, but it is not always satisfactory to divorce research on methods from the job of collecting, analysing and interpreting the data.

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III

I now pass on to consider some technical problems of a general character.

Discreetly in the background of the discussion about the need for up-to-date short period statistics is the question of what should be the role of the censuses of population, agriculture, industrial production and distribution, and more particularly what are the most appropriate arrangements for the timing of census activities, their inter-relationship, and what ought to be the relation between censuses and short period statistics. Economic statisticians like to have a regular flow of short period statistics to follow up and check on forecasts, and in turn, good census results to come along to support short period statistics. Census results on output, sales and deliveries provide the weights for various index numbers and the frames for subsequent sample enquiries. Statisticians who have had to struggle to reconcile data obtained from different sources can readily appreciate the blessing of having at least at intervals a complete return relating to one sector of the economy such as manufacturing, in which it is possible to relate immediately the data on output, employment and other economic characteristics one with another, but it is not always easy to convince those who supply the data of the value of censuses. Just one example of the need to consider the inter-relationship of censuses and short period statistics may serve as an illustration. It appears unnecessary to ask in a census of production for annual figures of physical output from firms already providing monthly or quarterly returns, but is it wise to make no check on the coverage of the short period returns?

This question of the relation of censuses to short period statistics leads on to more awkward ones of what should be the reporting unit from which returns are obtained and the implications of this choice on industrial classification. Should the reporting unit, to take one extreme, be the establishment defined as a separate technical unit at one address, or on the other extreme, should it be the enterprise possibly covering many units in different industries? On the choice of the reporting unit depends the source of much regional data and a great deal of the knowledge of the structure of industry, but this choice is not one wholly for the statistician to make. It may be possible to obtain information for the narrower establishment unit on employment, wages and perhaps, output. In collecting the newer kinds of series, for example, on stocks, capital expenditure and profits, the difficulties of getting information on an establishment basis become very great and indeed there may be good reasons why in any case the information should more properly be collected from enterprises and not from establishments. To compromise on agreeing on different reporting units for each class of statistics may only make it more difficult ultimately to combine and compare the statistical series prepared. If, for example, information is collected from enterprises on capital formation and profits, it is not easy to combine such information with data, say, on wages and salaries, collected separately from establishments. It may be all right for the broad totals for manufacturing industry but not for individual industries.

The idea of a standard industrial classification was first mooted to achieve greater comparability between figures of numbers employed and of output as shown in labour returns and censuses of population and in censuses of production. Its use has been extended into other fields of statistics such as capital expenditure, stocks, profits, and bank advances. Statisticians are now being urged to adopt a common classification for all economic statistics, and this makes the choice of the unit for reporting a live issue.

The difficulties which I have mentioned about correlating statistical series compiled

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from different sources of information and becoming available at different times, have raised again the major question whether it is not possible to devise a simpler system of collecting the key statistics needed from firms by asking for a composite return, say, monthly or quarterly. Although this idea seems attractive, experience so far shows that such a scheme is not practicable because all the data sought are not available at the same time, or even may not all be available in the records of particular establishments or enterprises. To ask for a composite return would mean that the replies would be delayed to obtain the most difficult figures to be entered in the return and there is a serious risk that the response rates would fall sharply. It has, however, as an alternative, been worth while to consider collecting information on one return for a number of items between which comparability is especially important, for example, deliveries and stocks, or hours worked and earnings.

As the data available have become more complete and more up to date, it has been possible to prepare regular estimates of national income and expenditure, at first annually and now for some items quarterly. I think British experience since the war has re-emphasized the fact that reasonably reliable estimates can only be built up if there is an organized system of detailed statistics behind them. It is dangerously easy to produce theoretical models in the form of social accounts and insert figures into tables which are really only papering over the vacuums of knowledge. The hard work comes in finding new sources of data and getting the figures to come in regularly.

But there is a real dilemma which arises in starting national income estimates as there is for other kinds of statistical series. If the economic statistician waited until he got all the data he wanted, it would be many years before the series could be prepared, and indeed the series might never be published at all because only by issuing provisional estimates can he most clearly show the kind of results likely to be obtained and the use which might be made of them, and thereby persuade people to provide the data needed. There is a further and added difficulty in improving national income estimates, namely, that they are presented in articulated tables, and revision of figures relating to one item, no matter what the reason—whether due to later figures becoming available, adjustments of past data in the light of census results or similar causes—immediately means consequential revisions to be made elsewhere throughout the tables. For example, if a census of retail distribution shows that for a past year estimates of retail sales to persons were say, too low, and thus personal consumption was shown too low, this may mean that personal incomes were estimated too low, or, if not, personal savings too high, or some other adjustment must be made to the income side of the tables. What is the most satisfactory way of introducing improved data for one item when work is going on at the same time to improve the reliability of other items? Even if the device of leaving an unexplained balancing item is used in the accounts, the statistician is likely to be asked privately where he thinks the corrections ought to be made. At what stage in the development of the economic statistics of a country should a start be made in publishing estimates of national income is a nice decision to make, balancing the advantages of encouraging the use of these estimates and therefore stimulating the demand for adequate data to improve the estimates against the disadvantages that the basic data may still not be good enough to let the unsuspecting place too much reliance upon them. After ten years I think we in the United Kingdom have reached the point at which in one sense it is more difficult to compile national income estimates than it was, because many of the gaps in the information needed have been filled and we have the task of reconciling different sets of data and not inferring what the data might be.

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IV

Against the background of these general remarks I should now like to refer to some of the items set out in Table A and point out a few of the problems posed in collecting the data needed.

(i) *Consumer expenditure.*—Ideally, what the user would like to have are figures showing the amounts spent on different kinds of goods and services, and these reclassified in different ways to distinguish, for example, expenditure on consumer durable goods. Lurking behind this request for information on consumer durables is the demand, sooner or later, for data on the replacement of these goods.

About half of the personal consumption is measured by statistics of retail trade. These statistics have been improved substantially in recent years but at the same time it has become clear that the statistics can be collected more conveniently, analysed by type of shop, and different types of shop may sell the same kinds of goods. Much ingenuity is therefore called for in trying to convert figures of sales collected by types of shops into figures analysed by kinds of goods sold.

Of other items of personal consumption not covered by retail sales, one of the most troublesome is expenditure on private motoring—the purchase of cars and their running expenses. Cars used for business or for personal use are the same make of cars, use the same kind of petrol and are licensed in the same way, and no one, except by the most exhaustive enquiries, can tell how much of the expenditure on motoring ought to be attributed to personal use and how much to business use. In consequence, some conventional approximations have to be made and these approximations vary from country to country. The statisticians in some countries indeed have given up trying to distinguish between the two. Some count all expenditure on cars and expenses as for business use. As a result, different countries are overstating or understating their estimates of consumer expenditure by unknown amounts.

One apparently obvious way of getting round these difficulties of trying to build up estimates of personal consumption from statistics of retail trade and by fitting together statistics of expenditure on individual goods and services is to go direct to households and ask them how much they are spending. If the sample of households is carefully drawn up and the non-sampling errors are not too large, it may be possible to gross up the results of sample enquiries to provide reasonable estimates of personal expenditure, at least for some items. No one has been able to do this satisfactorily yet but attempts will be made using the results of the continuing family budget enquiry.

(ii) *Capital formation.*—There are differences of view whether all or only part of expenditure on repairs and replacement of parts should be included in gross capital formation. The Scandinavian countries, for example, prefer a wider definition than we do in the United Kingdom. We have stuck more closely to the definition of what is charged to the capital account of trading companies.

In part this divergence of treatment is reflected in the kind of statistics used. The Scandinavian countries have built up their estimates of gross capital formation from statistics of output of capital goods and in so doing it is obviously easier to include all items, no matter whether they are used for repair, replacement or new installations. We in the United Kingdom prefer to get the information on capital expenditure direct from the firms acquiring the plant, machinery and buildings partly because it ensures that the definitions and timing of the figures of capital expenditure fit in more closely with other figures obtained

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from companies. Further, since firms are now being asked also to send in returns of their expected capital expenditure in the year ahead, it is sensible to get figures of current and expected capital expenditure on the same basis.

It may be argued that this discussion on how wide the definition of gross capital formation ought to be is partly a waste of time since what is more interesting are estimates of net capital formation. But as recent papers to our Society by Redfern and Barna have shown, it is not so easy to define even theoretically what net capital formation should be, let alone measure it satisfactorily.

(iii) *Stocks.*—It is known that changes in the level of stocks and work in progress are very sensitive to trading conditions but these changes are difficult to measure. There are so many commodities to be covered and stocks may be held at all stages in production and trade from the importer and the home manufacturer to the retail shop and final consumer. To get anything like a complete picture of stock movements means obtaining and keeping up to date detailed lists of traders and undertaking sample enquiries of great complexity.

Before the war relatively few figures were collected of stocks, and these were mainly for primary commodities. New series were started during the war of stocks of raw materials and finished products and many have been continued. Most of these figures are, however, of quantities and not in terms of value. Recently more attention has been paid to collecting figures not only of stocks but also of work in progress, analysed to show where they are held in the economy, since a change in business conditions may affect the level of stocks differently at various stages of production and trade. Firms, however, can usually only give figures of their total stocks and work in progress in terms of value and not by quantities. As a result two kinds of statistics are available; the first, stocks of some materials and products in quantities and the second, total stocks of materials and finished products and work in progress as recorded in the books of firms in terms of value. Methods have to be devised to fit together the information to provide a general analysis of changes in stocks.

Although total stocks and work in progress in the country at any one time are large—possibly about £8,000 million—it may eventually be possible to measure these total stocks reasonably well—as well as personal consumption or capital formation. Unfortunately for the statistician, for purposes of economic analysis the main interest is in changes in stocks and not total stocks, and a small error in the measurement of total stocks may be a large error in changes in stocks. From this point of view it is more important to have good figures of stocks than of some other items.

(iv) *Balance of payments.*—There has been much discussion in recent years about ways of presenting balance of payments estimates. One way has been set out in detail in the *Balance of Payments Manual of the International Monetary Fund* regarding the balance of payments as a record of all economic transactions between the residents of one country and the residents of another. An alternative way is to present the figures as the external sector of a system of national accounts, and for this the definitions and treatment of some items are made to accord with the principles of national accounting. In both cases the basic data used are, however, essentially the same, and it is mainly questions of re-arrangement which arise.

It is in the collection of the basic data where the practical difficulties appear. Two examples are shipping earnings and income of British companies operating abroad. British ships sail over all the seas of the world, on voyages which may last months, but to prepare short period estimates of balance of payments, some method has to be devised

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of estimating their earnings every quarter or half year. Again, the present *Manual of the International Monetary Fund* drawn up for preparation of balance of payments estimates suggests, for example, that in the case of branches and subsidiaries of companies operating abroad, their total profits should be taken into account, and not merely profits remitted. The undistributed profits of subsidiaries are to be counted as part of the gross income received for balance of payments purposes, and they are then transferred into the capital account and reckoned as overseas investment. The difficulties will be readily appreciated of keeping track of the activities of all British companies operating abroad and ascertaining how much of their profits they retain overseas for investment in those countries.

(v) *Industrial production.*—There is not now much disagreement on the technical methods of compiling monthly indices of industrial production but there is still room for argument on how wide a coverage a monthly index should have—for example, whether it should cover construction—and on how best to use the data which come in weekly, monthly, quarterly and annually, with differing periods of delay. There is a view that the objective ought to be an annual index of production extrapolated monthly for a period ahead using, if necessary, alternative series, and then the monthly series should be revised after the annual index for the next year is prepared. For the United Kingdom we have preferred to use all the data available at the time of preparation of a monthly index and to make continuous adjustments to the series.

Steps are now being taken to assimilate the methods of input/output analysis into the framework of national income accounts and especially into the study of short period changes in industrial production. The input/output approach can be used effectively for explaining changes in the index of industrial production in terms of changes in the various components of national expenditure—where changes in production have gone, whether for capital formation, exports or consumption.

(vi) *Prices.*—Recent work on the measurement of price changes had led to some fundamental discussions on the form and purpose of the index numbers of retail and wholesale prices and also shown the need for new calculations to deflate series such as the value of stocks and work in progress, capital formation and other items which appear in the national accounts.

In the revision of the retail price index number, a new attempt has been made to face up to the problem of trying to measure the housing component in the index, particularly to make adequate allowance for the housing cost of owner-occupiers, and also to deal with the problem of seasonal changes in consumption and prices. Two recent papers to our Society by Stafford and by Phillips have described the radical changes made in preparing index numbers of wholesale prices. The discussion on these has now moved to the meetings of the international agencies and largely centres on the merits of the net sector approach to measure the price movements of the inputs and outputs of sectors of industry.

(vii) *Financial statistics.*—It is perhaps not wholly by accident that this item, appearing at the end of the list in the table, until recently had not received the same amount of attention as other items. There has been much discussion on the content of some series, such as bank advances, capital issues and hire purchase finance, and much exploratory work done in improving estimates of personal savings either by getting information direct from households by interviews or by analysing the accounts of financial institutions. Such developments have been somewhat piecemeal and the renewed emphasis on monetary policy is prompting statisticians to have a fresh look at possible bases for a comprehensive scheme

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for the development of financial statistics, and in particular, whether the system of national accounts can be adapted in some ways for this purpose. Such a scheme should be broad in concept and cover the newer, as well as the older types of financial institutions.

In this search, one approach suggested would be to try to write down the kind of statistics which it is thought might be needed for the conduct of monetary policy over the years ahead, but it is realized that monetary policy may change in direction over time and such an approach should not be too pragmatic or too narrow in outlook. An alternative approach suggested is to see whether it is possible to measure the financial or money flows between different sectors—to create a flow of funds system of national accounts. If such a system was found to be useful in analysing the role of financial transactions in the economy, it might serve to indicate where, and in what directions, financial statistics might be improved.

The first attempts in this country towards measuring financial flows in this way were made in trying to study the sources of funds available for capital investment. These made obvious some of the difficulties to be encountered in a complete study of flow of funds, such as how to get figures on gross and not merely net, movements of funds in and out of different sectors of the economy; what considerations should govern the choice of sectors, particularly how financial institutions should be grouped, and how to assess the significance of the figures obtained without having analyses of liquid and fixed assets and of the liabilities of each sector.

V

Perhaps I have given enough examples to show the kind of problems which now face the economic statistician. There are others I am sure Fellows may think I ought to have mentioned but they all add up to putting much more responsibility on the economic statistician in his work of widening and deepening our knowledge of the structure of the economy and of changes in the economic conditions. We should be aware of what this involves.

The first task is to turn economic ideas into something that can be measured—to fill those empty economic boxes, as Clapham expressed it years ago. The development of economic theory, and particularly macro-economics, has led the economic statistician into fields where measurement is particularly difficult. His task is not made easier because the concepts which come to him are not always clearly stated, and indeed much of his work is in sorting out these ideas and re-expressing them in a form which makes them capable of measurement. At the same time he must be especially careful when deciding to measure economic phenomena in a particular way—maybe on grounds of statistical expediency—lest he strays too far away from the concepts the economist has in mind.

The need for a rapprochement between economists and statisticians is shown by the kind of articles now appearing on econometrics and mathematical economics. Many of these pose problems of measurement which look particularly difficult, if they are to be subjects to be tackled later by the statistician or for which the statistician will have to produce data to test out particular assumptions. On inter-industry relations, linear programming and demand analysis, steps have already been taken to get data to try out some of the methods of analysis proposed, but much exploratory work has still to be done if these ideas are to influence the lines of development of statistics in future years. There

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is a great deal to be gained in making the work of econometrics more allied to the study of economic statistics.

But as it has been remarked before, the job of the economic statistician has been more technical and at the same time much more administrative in character. The increased use of statistics in discussions of current economic policy and in planning of future operations has inevitably meant that he has been drawn in to take a part in the formulation and direction of economic policy. The statistician who has had to prepare the figures and is aware of their snags ought to be able to judge their significance and say how far it is possible to generalize from them. But often it is not so simple as that. Many economic statistics collected have to serve not one but a number of purposes, and the scope and definitions of the statistics do not satisfy all purposes equally well. Often at the time of decisions on policy questions the data then available are not those which exactly answer the questions raised, and the statistician may be asked to say what the answers might be, if in fact the data could be adjusted to try and answer the questions. This requires considerable judgment, no less than a thorough knowledge of the data, and the statistician must be prepared to say firmly whether an answer can or cannot be given. There is no doubt that better statistics are ultimately produced when the statistician is brought up sharply against the day-to-day use of the statistics he is preparing and a detailed knowledge of how they are used makes the statistician more able to anticipate the needs for different kinds of statistics.

The main task of the economic statistician, however, is still to see that the right kinds of statistics are collected. This may sound like saying the obvious, but it should be said because the taking of large surveys, which are a feature of economic statistics, is often such a big organizing job that it may be thought that it can be carried out wholly by persons other than statisticians, and indeed some statisticians feel their job is more in the analysis of results rather than the planning and conduct of the surveys. Recent developments have largely removed such doubts. Compared with the time when Flux, Stamp and Bowley started their work, many of the statistics now being collected are obtained from enquiries to be designed from start to finish as statistical enquiries. Moreover, the testing of questionnaires, the development of data processing equipment, the greater use of sampling methods and the checks needed for non-sampling errors, are among the reasons why an enquiry must be planned as one operation. To adapt words used by Sir Hector Hetherington, each such operation demands that the statistician should hold in mind the purposes of the enquiry and the form of the final results to be obtained, maybe a year or more later, should plan ahead imaginatively and firmly and yet with the necessary margins, should be on the look out for the unexpected as well as the expected in the results coming in, should have ready the staff and equipment at the right time, and be able to take consistent decisions on points of detail and indispensably to see that his colleagues know how to do the same.

VI

And so I end this Address but the development of economic statistics—the subject of my Address—goes on. There are things to do better and new problems ahead to challenge the skill of the statisticians. 1957 has seen the passing of Bowley—the last of our pioneers of the early years of this century in the development of economic statistics. Present Fellows take over from them the responsibility set out in the original aims of our

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Society in 1834, that we were established "for the purposes of procuring, arranging and publishing Facts calculated to illustrate the Condition and Prospects of Society".

PROCEEDINGS OF THE MEETING

Professor E. S. PEARSON (immediate Past President): It is with very great pleasure that I assume the traditional role of the retiring President and rise to propose the vote of thanks to our new President for the Address which he has delivered to us this evening. At the same time it is good to be able to welcome him on this, his first public appearance amongst us as President, for at the Annual General Meeting last summer when he might have been present to step into my shoes, he was engaged elsewhere as Chairman of the Conference of European Statisticians.

As Sir Harry has remarked, we are now members of a Society which incorporates a great and growing range of interests and there is no doubt that one of the problems of the future is whether we can hold together all these interests within a common bond. Our subject to-day gains or suffers—whichever way you like to look at it—from what may be described as its dual origin. One line of descent takes us back through generations of descriptive statisticians, among whom our founders were I think included, to where in the seventeenth century we find John Graunt seeking to extract information from the London Bills of Mortality. The other line is through the great French mathematicians, and it seems to run back to where mathematical scholars in sixteenth century Italy were struggling with the idea that a mathematical abstraction was to be found behind the observed results of games of chance.

Our President stands four square at the bottom of the first line of descent. To quote again the telling phrase from our Charter which he has read, under his guidance and with his encouragement the collection and presentation of "facts calculated to illustrate the condition and prospects of society" have indeed made very great steps forward. It is certain that the founders of this Society would have approved most highly of his work and of his Address to us to-day, while I fancy they would not have recognized your last President as a statistician at all!

I speak with great diffidence in this matter, for it is the first occasion—and perhaps the last—on which I have had to speak at a meeting concerned with the subject of Economic Statistics. It is fortunate, therefore, both for you and for me that custom strictly forbids me to comment on the subject of the Address. Where most of you here to-night have direct knowledge of the work of its author, I can only try to read back between the lines. Our President writes with great modesty and the casual reader would hardly know that he himself has been the architect of much that he describes. It was, I surmise, his persistence and patience and his disarming manner which led to a co-ordination in the collection and presentation of statistics by Government Departments, which simply did not exist before the War. In listening to his Address even a person as innocent of economic knowledge as myself can appreciate that great things have been achieved in the past ten years. Someone must have been holding the reins; I can only assume that it was the head of the Central Statistical Office!

When the War ended, the vast majority of university statisticians who had been drawn into the national service returned to their academic work and professorial chairs. Maybe the White Paper of 1944 on Employment Policy had set the sights for a new approach to the whole subject of national economic statistics, but a tremendous task of implementation, of planning and of persuasion lay ahead. In spite of tempting offers to draw him away, our President decided to stay on and carry through this vitally important work. It is, I judge, in recognition of his past achievements and in expectation of what he has yet to give, that we have elected him to the only chair which it is in our power to confer on him.

Lord PIERCY: I feel it a privilege to be able to second this vote of thanks.

Few statistical publications are so much thumbed over as the Blue Book on National Income and Expenditure—and by all sorts of people: politicians, economists and publicists

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of all kinds. None is so well thumbed over unless it might be the Monthly Digest of Statistics. Some part of the content in that was to be gathered from other sources before the war, but the compendious format of this encyclopaedic return is a great boon to people who are dealing in a practical way with commercial and industrial, not to mention political, issues. And now "the strong much toiling sage" to whom very largely we owe all that, and a good deal more, has given us this interesting account and review, which will rank among the most interesting of recent Presidential Addresses to this Society. He has dealt with the steps by which our present situation has developed, and it is useful to have this record of aims and achievement; he has set out some of the general considerations which have more or less formulated themselves in the carrying out of the programme; with some of the technical problems of a general character which emerge; and a number of the problems concerned with the content of what is collected and analysed.

It was salutary that he should quote the words from the Statistics of Trade Act stating its purpose: "to obtain information necessary for the appreciation of economic trends", because this is, so to speak, the charter of the collaboration of the economists and of the statisticians, of which he is a leading exponent; and it was the patient development of the programme arising out of that Act which as he says has created the new post-war attitude of the statistician towards the subject matter of economic statistics and has turned him into more of a professional character than he was before.

There is a great deal of sagacity in this Paper, and much food for thought. There is also a message of hope, because this alliance between the economist and the statistician is surely a novel and potent instrument of policy. The problems change their aspect and new solutions are required which may call for new bases of enquiry and even for new techniques. While we are grateful to the economists for their theoretical analyses and models, the realistic application of their science to the elucidation of problems of public policy must largely depend on the economic statistician, his techniques, his competence and his capacity for collaboration with the economists. It was one of the fundamental aims of the Society from the outset to promote such a collaboration.

The vote of thanks was put to the meeting and carried unanimously.

The PRESIDENT, in reply: I should like to thank Professor Pearson and Lord Piercy for the kind words they have spoken about my Paper. I hope that in restricting the paper to economic statistics I have not given the impression that social statistics, or indeed other classes of statistics, do not deserve as much attention as has been given in recent years to economic statistics. Personally, I feel that the too few who are working on social statistics need as much encouragement as can be given to them. I have spoken only of national statistics and not of all the efforts made to improve international statistics. I have done so deliberately because I have on a previous occasion addressed the Society on international statistics.

As a result of the ballot taken during the meeting the candidates named below were elected Fellows of the Society:

Sunmola Olatunji Adeyinka
Robert Rushby Armstrong
Lloyd Earl Assee
Paul André Baillod
Michael David Bakes
John Neville Banks
Myra Baum
Thomas Hugh Beech
Alan Duncan Begg
Sidney Benjamin
Peter John Bennellick
Richard Darcy Best
Walter Fred Bodmer
Harry Bramley
John Donne Burton
Donald Butler
Thomas Walter Cannell
Dorothy Selina Chalk
Tse Fo Chang
Francis John Chatterley
Peter John Claringbold
Kenneth Edward Clark
John Burnett Cox
John Roberts Davis

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Vishnu Prasad Dhital
Alan Gordon Richard Dobbs
James Bertram Douglas
David Conochie Duncan
Joyce Dyson
James Reginald Carrington Elmslie
George Joseph Feeney
Stephen Trevor Feldman
Mereth Cecil Fessey
Bernard Hayden Fison
Rupert Fitzpatrick
Anthony David John Flowerdew
Jean Forbes
Marjorie Fowler
Ivor Garland
Merritt Milford Goff
Ronald William Green
Shugan Chand Gupta
Alan Ernest Charles Hards
Henry Roy Harris
Peter Jeffrey Harrison
Ralph Thomas Hart
Mary Georgina Hills
David Boss Hunter
Jia Lal Jain
André Michel Johnson
Roy Andrew Jones
Basil Charles Kahan
Peter James Kendell
Sidney King
Ernest George Knox
Norman Lamont Lawrie
Peter Lewis
Judith Mary Lloyd
Brian Owen Longman
Clive Loveluck
Michael Donald McCarthy
Murdo John Maciver
William Mendenhall
Ross Donald Munro
Eric Nixon
William Oldfield
Gloria May Overton
Maurice Pace
Peter Demetrius Pappas
Angela Hazel Alberta Penny
George Henry Peters
Jack Potter
James Nelson Proctor
Jean Rosemary Proctor
John Fuller Ratcliffe
Turayur Ananthan Ramasubban
Gulab Nanikram Ramchandani
Olav Reiersøl
Lucy Rigby
Sally Elizabeth Acland Rose
Raymond Roth
Derek Jarvis Saffell
Donald Curtis Shaw
Leonard Walter Shaw
Amar Krishna Sirker
Raden Soedarjono
Brian Curtis Stimpson
Russi Jal Taraporevala
David Anthony Hayden Thomas
Sonja Thomas
Robert Basil Trollope
Shawkat Ahmad Tuffaha
David Valinsky
Norman Thomas Wagstaffe
Dennis Walker
John James Blake Ware
Geoffrey Anton Watterson
Barbara Elizabeth Anne Wheller
Eric John Whyte
Leonard Williams
Charles William Winterford
Vivian Leopold Alexander Wong

Corporate Representatives

David Earle Bailey, *representing* A. V. Roe & Co. Ltd., Weapons Research Division.
Arthur James Barrow, *representing* Hogg & Mitchell Ltd.
Anne Katherine Jackson, *representing* the National Institute of Economic & Social Research.
Laurence Kevin O'Connor, *representing* the Milk Marketing Board of England and Wales.
Geoffrey James Eveleigh Rhodes, *representing* Hulton Press Ltd.

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The Depreciation and Mortality of Motor-Cars*

By J. S. Cramer

[Read before the Royal Statistical Society on December 17th, 1957, the President, Sir Harry Campion, C.B., C.B.E., in the Chair.]

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1. Introduction

The depreciation of durable consumer goods is not a precise concept. In a general sense the depreciation of capital goods used in production can be said to describe how such goods are wearing out while they are yielding a continuous flow of productive services. It is then a matter of costing and accounting methods to ensure that the decline of their current value corresponds to the services rendered during the period. But even this general and rather naive view of depreciation cannot be applied to consumer goods, if only because there are no grounds for relating the satisfaction which the owner derives from them with the decline in value they suffer.

The absence of a theoretical framework, however, does not affect the fact that consumer durables deteriorate and that their value declines the older they become. We shall not try to define this process further, but we shall instead take it for granted that there is such a thing as the quantity or value of a consumer durable, and that it declines with age. The object of the present paper is then to present a statistical model for this process, and to examine its agreement with related observable phenomena in the particular case of motor-cars. These phenomena are secondhand prices and mortality or scrappage, both being considered as functions of car age.

It will be noticed that depreciation is considered in relation to age only, and that in the actual applications of the model there is, therefore, no room to account for the mileage of a car, the care which it receives or the repairs which are undertaken. These factors are neglected.

In the usual type of depreciation model a fixed depreciation function is assumed to hold for all items under review. Such a deterministic approach appears to imply that all the items of the same age would have to be replaced at the same time. This is contrary to experience, which suggests that replacement of similar goods is spread over a considerable range of ages. In order to provide for this, depreciation is, in the present model, treated as

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a chance event. Both the quantity (or value) which a car retains and the depreciation it suffers over a given period are random variables.

Two possible causes of depreciation are distinguished. The first consists in the loss of small fractions of value or quantity, one at a time, and corresponds to gradual wear and tear. Secondly, there is a constant risk that a car, whatever its age or condition, is suddenly altogether destroyed, e.g. by an accident. Together these two factors define a very simple stochastic process which in turn generates a series of probability distributions for the quantity retained by motor-cars at successive ages. These distributions are briefly examined in section 2.

To permit an application to observed data further assumptions must be made. The first is that secondhand car prices are determined by depreciation. This opens the way for a large-scale application of the model and for the estimation of one of its parameters (the rate of gradual wear) from a variety of data. There are precedents for measuring car depreciation by secondhand prices (Boiteux, 1956; Smith, 1941); but it is seldom realized that unless its influence is traced through the mechanism of demand and supply, such a procedure is contrary to economic theory. Nevertheless we, too, have adopted the device without providing such a theoretical justification.

The second application is to mortality rates and rests on the assumption that a car is scrapped when it reaches zero quantity. The probability densities at this point can then be interpreted as a cumulative mortality function. Mortality tables for durable goods have, of course, been studied earlier (Kurtz, 1930; Pennock and Jaeger, 1957; Boulding, 1955). But such studies have usually been concerned with tabulation and mere description rather than interpretation of the observed curves. The main interest of the present results is in their relation to the estimates obtained from secondhand prices. It is found that there is some agreement between the two, and this leads finally to a discussion of the present situation in the United Kingdom which is characterized by very low scrappage rates.

In so far as it provides a description of these two hitherto unrelated phenomena by a single model the analytical performance of the model is satisfactory. To establish this connection between prices and mortality rates on a theoretical basis may be regarded as the main object of the paper, and in other respects it contains little of interest. The estimation, for instance, is based throughout on simple least squares.

There remains a problem of interpretation. The discussion is throughout formulated in terms of physical wear and tear, but an alternative interpretation is possible. For it may be that scrappage depends on the secondhand price of a car, and that economic forces underlie the observed mortality. Prices rather than some undefined quantity or value concept would then constitute the final element of the analysis. Such an interpretation provides a direct link to the observed agreement between secondhand prices and car scrappage rates.

This raises the wider question whether the present model could be integrated in a general economic model of durable consumer goods. One would like to follow Bandeen (1957) and consider the depreciation during a particular period as equivalent to the consumption which the car's owner has enjoyed. But such an interpretation raises difficult theoretical problems which are connected with the indivisibility of most durable consumer goods. A solution of these problems is outside the scope of the present paper. The formulation of the model is in physical terms, which seemed less confusing than the use of value concepts; but the ultimate explanation of the phenomena under review must be left undecided.

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2. A Stochastic Model of Car Depreciation

The starting point of the present model is that there is a quantity measure associated with cars so that each represents a definite amount of, say, the commodity "car". Depreciation is measured by this quantity, and it does not matter here whether it is identified with "value", "wear", "performance" or some other similar aspect of a car.

It will be convenient to treat this quantity as a discrete variable which is restricted to ν equidistant values. If an appropriate unit of measurement is chosen it can then be expressed as a number of units j, $j = 0, 1, \ldots \nu$. The unit is defined as the νth part of a new car's quantity and j therefore equals ν for all new cars.

It is next assumed that each of these units is subject to the same constant risk of breakdown or loss. More precisely, the probability for any particular unit to disappear during an interval dt is proportional to its duration but independent of anything else; or it is

$$\lambda \, . \, dt.$$

Hence for a car which consists of j units the probability that its quantity declines to $(j - 1)$ during the interval is

$$j\lambda \, dt. \qquad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

These probabilities hold for small intervals only; in fact, the periods are assumed to be so short that the probability of losing more than one unit in the same interval can be neglected.

This risk of losing single units, one at a time, may be taken to represent gradual wear and tear. Besides, we should allow for accidents in which a car may be completely destroyed, i.e. its quantity is suddenly reduced to zero. It is again assumed that the probability of such an occurrence in the interval dt is proportional to its duration, and that it is independent of the car's age or quantity. Any car may therefore be reduced to zero quantity with a probability, of, say,

$$\mu \, dt. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

These two assumptions together define a very simple stochastic process. The effect of the first on car quantity is equivalent to that of a constant death rate in a population life and death process, and the second introduces no fundamental complications. Explicit expressions for the probability distribution of the quantity of a car at any given age can therefore be derived by a straightforward application of the Chapman-Kolmogorov equation (Feller, 1950, especially ch. 17). If $P(j, t)$ denotes the probability that a car of age t has quantity j, the relevant expression for non-zero quantities is

$$P(j, t) = e^{-\mu t} \binom{\nu}{j} (e^{-\lambda t})^j (1 - e^{-\lambda t})^{\nu - j} \qquad 0 < j \leqslant \nu. \quad . \quad . \quad (3)$$

For zero quantity the density at age t is found to be

$$P(o, t) = 1 - e^{-\mu t}\{1 - (1 - e^{-\lambda t})^{\nu}\}. \quad . \quad . \quad . \quad . \quad (4)$$

The series of probability distributions of car quantity at successive ages defined by these equations is shown in Fig. 1. At zero age, the mass of the distribution is wholly concentrated at $j = \nu$, the initial number of units of new cars. At later ages car quantities become more widely dispersed and their mean declines. For a given age, the distribution by quantity turns out to be a slight modification of the binomial. It is seen from (3)

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that for non-zero quantities the density is proportional to that in a binomial distribution with $n = \nu$ and $p = e^{-\lambda t}$. All densities are, however, proportionately reduced by the factor $e^{-\mu t}$, and this reduction is offset by a corresponding increase in the element for $j = 0$. Consequently the composite distribution for $j = 0 \quad . \quad . \quad . \quad \nu$ may well be bimodal for moderate values of t.

Generally, however, we will make a distinction between the two parts of the distribution—the densities for $j = 0$ and for $j \neq 0$ respectively—and in what follows they will usually be considered separately. This is because the two parts are identified with different phenomena. When introducing the parameter μ we have already related the loss of all

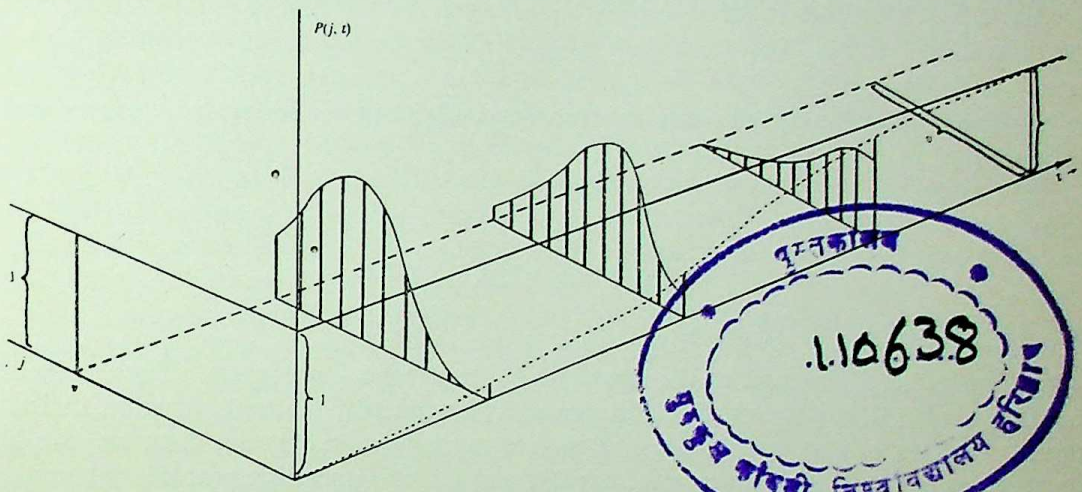

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FIG. 1.—The function $p(j, t)$ defined by equations (3) and (4).

the quantity of a car to its destruction in an accident. Extending this interpretation we will now identify any passage to zero quantity with the scrapping of a car. This may occur as a result of an accident which is connected with μ, but most cars will be scrapped after gradually approaching complete obsolescence; that is, in most cases the last unit will be lost because of the operation of λ.

It follows from this interpretation that equation (4) gives the probability that a car has been scrapped at a given age; applied to a generation of cars, it indicates the frequency of scrapped cars at each age, and is thus equivalent to a cumulative mortality function. The distribution of non-zero quantities given by (3), on the other hand, exclusively describes the quantity distribution of surviving cars.

The Quantity Distribution Among Surviving Cars

We will first examine this latter part of the distribution. If (3) is corrected for the declining size of the surviving part of a given car generation, the expression

$$P(j, t \mid j \neq 0) = \binom{\nu}{j} \frac{(e^{-\lambda t})^j (1 - e^{-\lambda t})^{\nu - j}}{1 - (1 - e^{-\lambda t})^{\nu}} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

is found to describe the quantity distribution of surviving cars of any given age t. The parameter μ does not enter into this function which specifies a truncated binomial distribu-

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tion with $p = e^{-\lambda t}$, $n = \nu$, the truncation occurring at $j = 0$. As t increases, $p = e^{-\lambda t}$ declines from unity to zero, and in the original binomial this leads to a neat pattern of simultaneous changes in its characteristics. The mean declines with p; the variance increases to a maximum at $p = \frac{1}{2}$ and declines afterwards; and it is negatively skewed at first, symmetrical with maximum variance, and positively skewed in the end. All this is affected by the truncation; but as a matter of fact we shall find it to be negligibly small, at least for that part of a car's life for which this distribution will be considered.

Up to now car quantities have been measured in the rather arbitrary units introduced for the purpose; but in the applications we will express the quantity of a car of any age as a fraction of its initial quantity when new. We will thus consider the variate

$$x = \frac{j}{\nu}$$

rather than j. For the period where the truncation effect can be disregarded, its mean and variance are

$$\mathscr{E}(x/t) \sim e^{-\lambda t} \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

and

$$\text{Var}\,(x/t) \sim \frac{1}{\nu}(e^{-\lambda t})(1 - e^{-\lambda t}). \quad . \quad . \quad . \quad . \quad (7)$$

The Distribution of Car Mortality

We now turn to the part of the model which describes car scrappage. The density at $j = 0$, given in (4), has already been interpreted as a cumulative mortality function, indicating the fraction of cars which does not survive to a given age. It can therefore also be regarded as a distribution function of car life, and the corresponding probability distribution is (by differentiation)

$$\text{dF}(l) = e^{-\mu t}\{\mu - \mu(1 - e^{-\lambda t})^{\nu} + \lambda\nu(e^{-\lambda t})(1 - e^{-\lambda t})^{\nu-1}\}\, dt. \quad . \quad . \quad (8)$$

In the case $\mu = 0$ this reduces to a transformation of a B-distribution with parameters $(1, \nu)$; as μ is, in fact, quite small, the shape of the distribution will be not much different.

The main characteristics of (8) can be derived from its moment generating function which is

$$m(k, t) = 1 - \frac{k}{k - \mu} - \frac{k}{\lambda} B\left(\frac{\mu - k}{\lambda}, \nu + 1\right). \quad . \quad . \quad . \quad (9)$$

It follows that its mean, i.e. the expected life of a new car, is given by

$$\mathscr{E}(l) = \frac{1 - Z}{\mu} \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

and its variance by

$$\text{Var}\,(l) = \frac{1}{\mu^2}[1 + Z\{2\theta(\psi(1 - \theta) - \psi(\nu + 1 + \theta)) - Z\}] \quad . \quad . \quad (11)$$

where

$$\theta = \frac{\mu}{\lambda} \text{ and } Z = \theta \,.\, B(\theta, \nu + 1)$$

while

$$\psi(x) = \frac{d \log \Gamma(x)}{dx}.$$

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These expressions permit the evaluation of both characteristics for any given values of the parameters.

Interpretation of the Parameters

The interpretation of the parameters λ and μ does not present great problems; they have already been identified with the rate of gradual wear and the accident rate. Clearly we may expect μ to be the same for all cars, while λ will generally vary for different makes and models.

The third parameter ν is in a different category in that it does not correspond to a single aspect of the process we are describing. At first sight it may seem an arbitrary constant, which would disappear if only the model were formulated in a continuous rather than the present discrete version. But this is not so. Even though the numerical value of ν can be eliminated from the measure of car quantities (as has already been done by substituting x) it is still required for an adequate description of the process we are considering. It is operative in the degree of truncation and the variance of the survivors' distribution and enters in all the characteristics of the car life distribution. Even in a continuous approximation to the present model, therefore, a corresponding parameter would have to be introduced to control these characteristics.

In the present model the quantity of a car (and thus its depreciation) is a random variable, and it differs therefore from the familiar deterministic depreciation functions. It has an analogue, however, in the "reduced balance" depreciation formula where depreciation is proportional to the current (written-down) value of the asset. This has been applied to consumer goods by Stone and Rowe (1957) with satisfactory results. The correspondence between the two formulae is best seen by considering a car which consists of t units at age t. It follows directly from (1) and (2) that its expected quantity at age $(t + dt)$ is

$$j\{1 - (\mu + \lambda)\, dt\}$$

so that the expected depreciation during the interval dt is proportional to j with a depreciation rate of $(\mu + \lambda)$.

3. The Behaviour of Secondhand Prices

Having specified the distribution of car quantity among the surviving cars of any given age, we now make the further assumption that these quantities are reflected in secondhand prices. More precisely, we suppose that the secondhand price of a car consists of two distinct parts. The first, say α, corresponds to the scrap value of the car and is constant, independent of its age or quantity. The second part represents the value-in-use of the car and is proportional to its quantity x. If we require the same relation to hold for the list price p_0 of a new car, for which x equals 1, the general formula for secondhand prices at age t must be

$$p^*(t) = (p_0 - \alpha)\, x + \alpha \quad . \quad . \quad . \quad . \quad . \quad (12)$$

x being a random variable the distribution of which depends on t.

This equation may briefly be compared with the depreciation formula which has recently been derived by Boiteux (1956) from French secondhand car prices. After establishing that these prices follow the same regular decline for cars of different vintage, Boiteux

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examines their first differences and fits a frankly heuristic curve of the form (in our notation)

$$\frac{p^*(t) - p^*(t+1)}{p_0} = \frac{k}{t} \quad *$$

where k is a constant which assumes values of about ·30 to ·25. The corresponding equation of the present model is (12) rewritten as

$$\frac{p^*(t) - p^*(t+1)}{p_0 - \alpha} = e^{-\lambda t}\{1 - e^{-\lambda}\}.$$

For a comparison of this kind α may be equated to zero; and, for the pre-war period, we will find values of λ which lead to a factor $(1 - e^{-\lambda})$ of about the same magnitude as Boiteux's k. The main difference between the two models is then whether the decline in successive annual depreciation percentages is described by a parabola or by an exponential.

Whatever its precise form, however, any model which equates depreciation to the development of secondhand prices is contrary to economic theory. For economic theory holds that prices depend primarily on demand and supply, and not on the age or quantity of the car concerned; such factors only influence prices in so far as they condition demand. For on a secondhand market the potential supply is given and it is therefore possible to have a model of price determination in which demand factors alone vary. An analysis of secondhand car prices which is based on such considerations has been given by Farrell (1954). He distinguishes between cars of different age-groups and treats each as a separate commodity, qualitatively different from all other age-groups or, for that matter, from new cars. Although the substitution between the adjacent age-groups is an essential part of this model, there are separate demand functions for each, and the relation between them is subject only to the condition that at the same price a newer car is always preferred to an older one.

The present assumption that price depends on quantity, which varies in a prescribed way with age, may be regarded as an elaboration of this preference hypothesis. From this point of view the present model lays down such a precise specification of the demand side that there is no room left for supply factors to have any effect on prices at all. This is perhaps justified for the particular data which we shall use. For instead of aggregate age-groups for the whole car population we shall consider the successive generations of quite distinct types of car. It can be argued that in that case the physical condition of cars—and its evaluation on the demand side—is a more important determinant of price differentials between age-groups than the number of each in existence. A secondhand car of a given age-group which belongs to a certain type, specified by model, make, etc. may still strictly be a distinct commodity with a demand curve of its own and a given supply. But precisely because of its narrow definition there are many close substitutes which diminish the importance of the potential supply. In fact, the relative valuation of similar cars of different ages may be expected to depend primarily on the wear and tear associated with age; and this is what we are assuming.

* Mr. H. R. Fisher has pointed out to me that this equation implies an infinite depreciation for the first year of a car's life. Although Boiteux actually gives the equation in the present form it appears from his applications that the right-hand side should in fact read

$$\frac{k}{t+1}$$

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Application to Pre-war Secondhand Prices in the United Kingdom.

In order to find out whether equation (12) is at all capable of explaining actual prices it has been applied to a large volume of data pertaining to the British secondhand market in the period 1932–39. Monthly data for a wide range of different cars have been published for those years in *Glass's Guide to the Used Car Market*, which, for each car, provides an analysis of the prices quoted in advertisements. Four characteristics are given, viz. (i) the number of advertisements, (ii) the highest and the lowest quotation and (iii) the average of all prices quoted. These last observations have been taken to correspond to the mean secondhand price of all surviving cars of a given type and age, which according to (6) and (12) is given by

$$\mathscr{E}(p(t)) = (p_0 - \alpha)\, e^{-\lambda t} + \alpha. \quad . \quad . \quad . \quad . \quad . \quad (13)$$

This equation has been fitted to time-series of the average secondhand prices recorded at three monthly intervals. As we are primarily interested in relative prices of cars of different ages, this procedure is strictly acceptable only in so far as the level of new car prices has remained constant during the period under review. Taking quality changes into account there was presumably in fact a slight downward trend which may lead to an upward bias in the estimates of λ. But whereas a "cross-section" analysis of relative prices at any one moment would be restricted to five or six observations, the time-series provide fifteen to twenty-five observations each. These series begin generally in the fourth quarter after the introduction of a car, as earlier observations on secondhand price (if available) seemed too unreliable. It would moreover be difficult to assess the age of the cars concerned. For the later observations age was equated to the time which had elapsed since the middle of the "model year" by which the cars are dated.

Equation (13) has been fitted to these series by simple least squares procedure; we have thus implicitly assumed an additive random error term. Various considerations could have been adduced in favour of a more sophisticated statistical model. We might allow, for example, for the different sample sizes which are known for each of the observations; or we might explicitly take into account the correlation between the disturbances and the level of the secondhand price concerned. No clear indication emerges, however, from a closer examination of these partially conflicting factors, and in the end the present method was adopted for simplicity's sake. The only serious objection to it is that it does not allow for serial correlation of successive error terms which is certainly present in so far as they reflect neglected economic factors. Some of this (and also some of the serial correlation among the actual observations) may have been avoided by taking every third of the monthly observations only.

Even the simple least squares assumptions which neglect these complications involve extensive computations. Because of the non-linearity of (13) recourse must be had to the method of scoring which arrives at the required estimates by successive approximations (Rao, 1952, especially ch. 4). No further adjustments were made when both corrections simultaneously satisfied the inequalities

$$|\Delta\lambda| < \cdot 002, \qquad |\Delta\alpha| < \cdot 05.$$

These correction terms apply to estimates of the depreciation rate λ measured as a fraction per annum and of the scrap value α in £. The computations were carried out on EDSAC I.

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These calculations have been made for a large number of individual cars, details of which are given in Table 1. It will be seen that there are fifteen different models which cover a wide price range. Seven of these represent the typical mass-produced family saloon of the period. For each car the consecutive models of 1931–34 have been included in so far as they were available. In some cases the four models are virtually the same, but changes in design do occur, in particular among the higher-priced cars. The number of observations of the time-series varies mainly because of the different dates of introduction.

TABLE 1

Description of Secondhand Price Data

Price Range When New (£)	*Description of Car*	*Number of Observations* 1931 *Model*	1932 *Model*	1933 *Model*	1934 *Model*
115–130	Austin 7 fixed head saloon "Ruby"	19	25	22	18
120	Ford 8 saloon "Tudor"	..	..	22	18
155–160	Hillman Minx saloon "Family"	..	25	22	17
155–160	Austin 10 fixed head saloon "Lichfield"	..	..	22	18
168–173	Austin 10 the same "Lichfield de Luxe"	..	23	22	18
180–185	Morris 12/4 saloon "Cowley"	19	25	22	18
200	Wolseley 12/6 saloon "Hornet"	..	23	22	18
310–325	S.S.I. 16 h.p. drophead coupé	..	23	22	17
450–495	Alvis 12/50 semi-panelled "Firefly"	18	25	22	18
485–505	Buick light 8 saloon "Viceroy"	..	22	21	17
495–585	Talbot "75" (short ch.) sports saloon	15	17	22	18
735	Humber 24/6 Limousine "Pullman"	19	20	22	17
950	Lagonda 4·5 l. "Pillarless" saloon	..	..	..	18
1460	Bentley 3·5 l. "Park Ward" saloon	..	..	..	18
2500–3100	Rolls Royce 40/50 Sedanca de Ville	11	*	15	*

.. Model year does not exist or is not listed in *Glass's Guide*.
* Rolls Royce's 1932 and 1934 models, for which 5 resp. 8 observations are available, have been omitted.

The results are set out in Table 2, which gives the estimates of λ and α, the squared correlation coefficient R^2, and s, an estimate of the residual standard error. The last two statistics are based on the second moment of the actual residuals rather than on their variance; as the fitted curve does not allow for an additive constant term, the two may differ even though the expected mean error is of course zero.

Our first concern is with the fit of the model to the observations. In this respect the results are satisfactory. The residual variation is small, at least in relation to the magnitude of the prices concerned; it was to be expected that the absolute size of the errors would increase for the higher priced cars. The high values of the correlation coefficient must be regarded with some reserve; as the observed secondhand prices show a steady decline, any model which would account for this development must lead to a considerable reduction in the observed variance. In fact, however, the present model does reproduce the particular curvature of the observed relation, as can be seen in Fig. 2. On the whole it can be said to correspond well to the secondhand price series considered; it does less well for the most expensive cars, Bentley and Rolls Royce, but here the observations are few and each of them is based on a small number of advertisements only.

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TABLE 2

Estimates for Equation (13)

Car Make and Model Year		λ *Depreciation Rate (Per Annum)*	a *Residual Value (in £)*	R^2	s *(in £)*
Austin 7	1931	·384	20·2	·965	2·1
	1932	·371	20·6	·971	2·7
	1933	·427	28·8	·964	2·7
	1934	·555	37·0	·941	2·9
Ford 8	1933	·431	22·8	·982	2·2
	1934	·544	31·6	·982	1·9
Hillman Minx	1932	·233	−6·9	·972	4·6
	1933	·382	21·4	·982	3·2
	1934	·375	16·2	·933	5·1
Austin 10	1933	·387	27·4	·956	4·9
	1934	·501	38·7	·943	4·2
Austin 10 de Luxe	1932	·294	12·1	·993	2·2
	1933	·326	20·9	·996	1·6
	1934	·466	39·7	·984	2·7
Morris 12/4	1931	·476	15·1	·931	4·0
	1932	·467	14·8	·973	4·0
	1933	·554	23·1	·975	3·8
	1934	·425	21·9	·976	3·9
Wolseley 12/6	1932	·338	2·2	·996	1·9
	1933	·433	16·1	·994	2·4
	1934	·492	31·0	·984	3·3
S.S. 16 h.p.	1932	·416	10·4	·976	7·1
	1933	·382	·8	·996	3·8
	1934	·395	−1·0	·965	10·3
Alvis 12/50	1931	·379	60·3	·861	20·8
	1932	·214	−70·3	·966	16·3
	1933	·326	5·0	·982	12·3
	1934	·429	66·9	·967	13·1
Buick 8	1932	·413	·4	·939	22·5
	1933	·313	−42·5	·989	9·4
	1934	·321	−65·5	·985	12·2
Talbot "75"	1931	·433	−2·8	·933	13·8
	1932	·340	−89·3	·953	21·5
	1933	·313	−59·7	·941	26·0
	1934	·418	−13·6	·986	10·3
Humber 24/6	1931	·495	73·6	·955	12·0
	1932	·396	34·3	·890	29·3
	1933	·356	21·5	·965	22·7
	1934	·454	114·0	·917	36·5
Lagonda	1934	·360	−35·6	·990	15·9
Bentley	1934	·037	−4569·3	·935	68·1
Rolls Royce	1931	·163	−1216·2	·709	153·3
	1933	·389	588·1	·746	256·1

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The estimates of the parameters are less successful. The rate of depreciation varies considerably from one car to another, and even from one annual model of a particular car to the next. And the same holds for the estimates of the residual scrap value of the cars. In several cases this variable assumes negative values which are clearly unreasonable. On closer examination it becomes apparent that the variations of these two parameters are related to each other, and that a comparatively high value for the one will entail a high estimate for the other. Inspection of equation (13) shows that this is, after all, to be expected; the same overall rate of decline of secondhand prices may either be described by a high value of both λ and α or by low values of the two parameters. The exact values

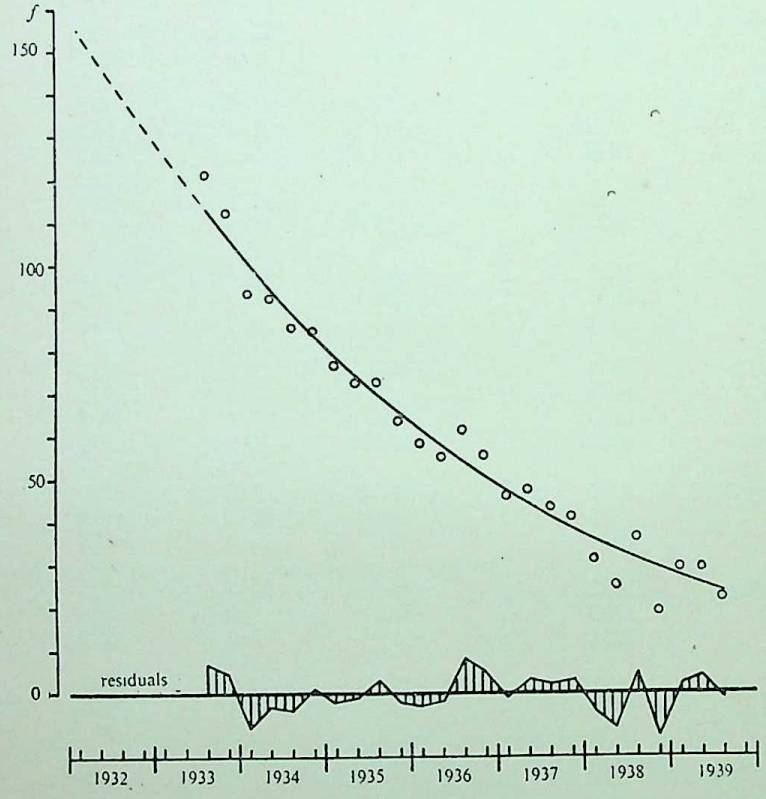

FIG. 2.—Observed and calculated secondhand prices of the 1932 Hillman Minx.

of the pair of parameters within this range of joint variation will be determined by the particular curvature of the observations rather than by their general tendency; and the ultimate location of our estimates within the (λ, α) plane will therefore be quite sensitive to small changes in the curvature of the observed series.

It is clear that we must allow for this in a discussion of the differences between the various pairs of estimates, and should take their covariation into account. In the present case, where only two parameters are involved, the joint distribution of the estimates can be examined graphically. As an example the joint confidence regions of the pairs of (λ, α) estimates for the three Hillman cars we have considered are shown in Fig. 3. The coefficients of these ellipses are derived from the elements of the information matrix, which

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also enters into the iterative estimation procedure we have adopted. The 95 per cent. confidence regions are defined by the condition that the quadratic form of joint maximum likelihood estimates is chi-square distributed; the present discussion is therefore contingent upon the assumption that our least-squares estimates are, in fact, maximum likelihood estimates.

Fig. 3 shows that, in effect, there is correlation between the estimates of the two parameters; the confidence regions are long, thin ellipses. Moreover this correlation does

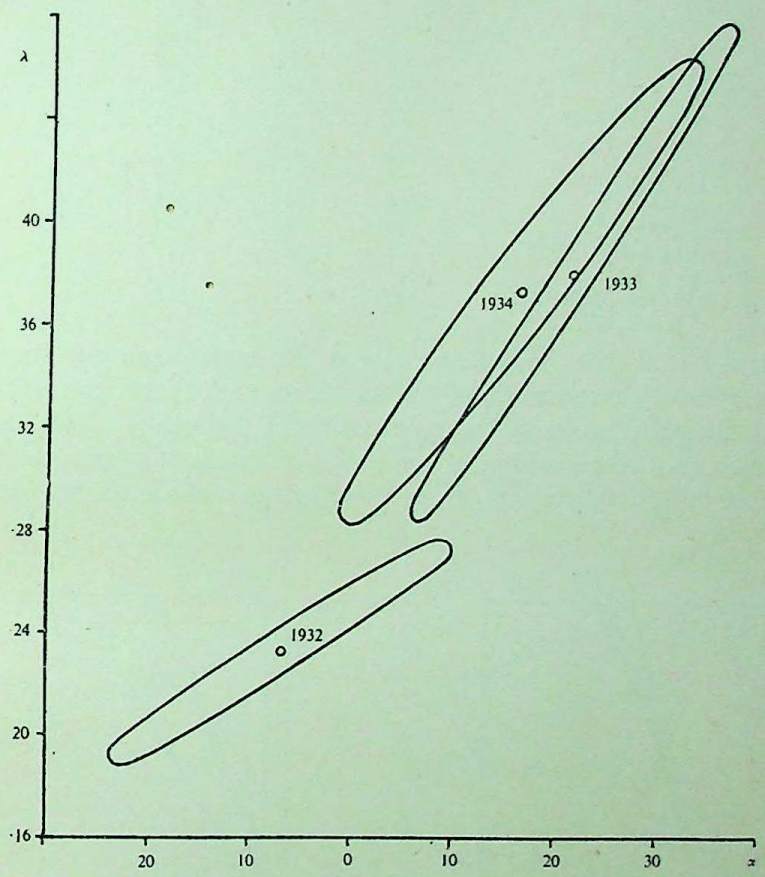

FIG. 3.—Confidence regions for the parameters λ and α; Hillman Minx.

considerably reduce the differences between the separate pairs of estimates; for the principal direction of the ellipses corresponds to the direction of the straight lines which would join up the pairs of point estimates themselves. Even so, at the adopted level of significance the estimates for the 1932 model are different from the two other pairs.

Similar relations obtain for most other cars. Complete confidence regions have been determined for the three cars shown in Fig. 3 only, but the correlation coefficients $\rho_{\lambda\alpha}$ of the estimates which determine the shape of these ellipses were more easily derived. They are given in Table 3. All have high values, and it appears that the Hillman cars shown are by no means exceptional in this respect.

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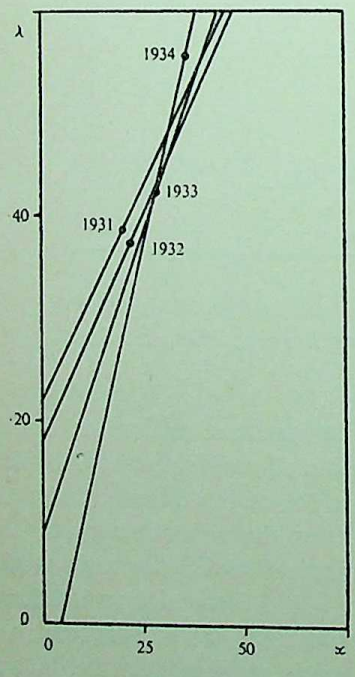
TABLE 3

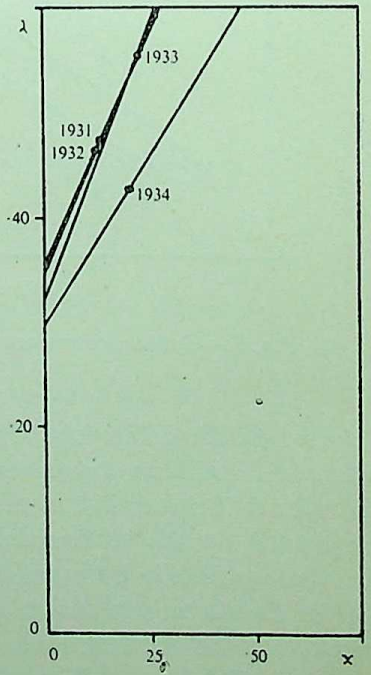
Correlation Coefficients of the Joint Distribution of the Estimates of α and λ

Car Type	*Value of $\rho_{\lambda\alpha}$*			
	1931 *Model*	1932 *Model*	1933 *Model*	1934 *Model*
Austin 7	·951	·930	·930	·916
Ford 8.	..	..	·928	·920
Hillman Minx	..	·976	·945	·965
Austin 10	..	..	·943	·933
Austin 10 de Luxe	..	·967	·962	·943
Morris 12/4	·921	·886	·878	·954
Wolseley 12/6	..	·952	·927	·936
S.S. 16 h.p.	..	·923	·945	·959
Alvis 12/50	·954	·980	·962	·953
Buick 8	..	·934	·969	·979
Talbot "75"	·959	·955	·965	·956
Humber 24/6	·914	·943	·953	·949
Lagonda 4·5 l.	..	..	..	·969
Bentley 3·5 l.	..	..	..	·999
Rolls Royce 40/50	·998	..	·949	..

Similarly, it is found that the Hillman case is also representative in its other aspects, and that it is a general characteristic of the confidence regions that their direction agrees fairly well with the distribution of the point estimates themselves. This can be seen in Figs. 4*a*–4*e* which show the position of the (λ, α) estimates of successive models of the same car together with the direction of the principal axis of the ellipse associated with each pair.



(*a*)



(*b*)

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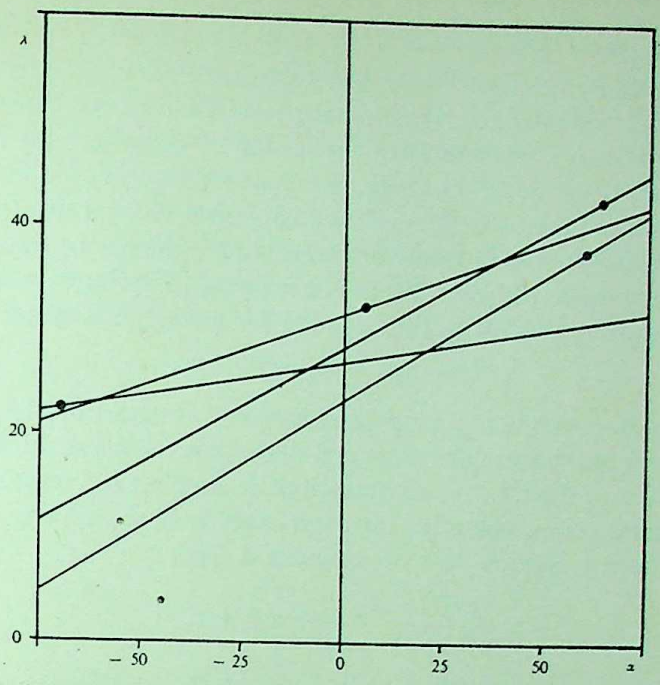

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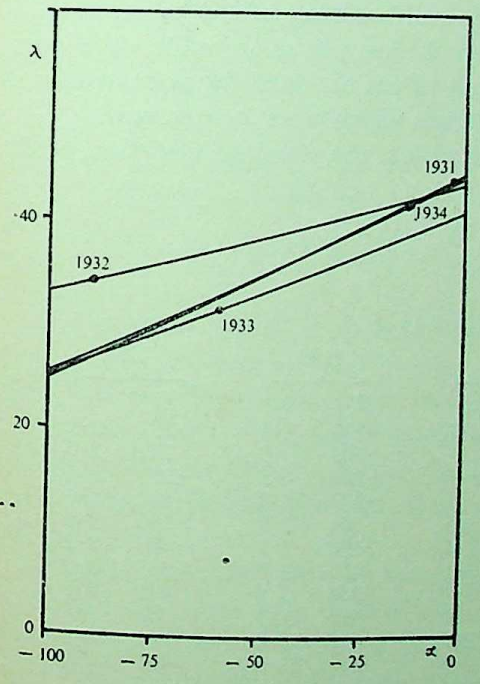

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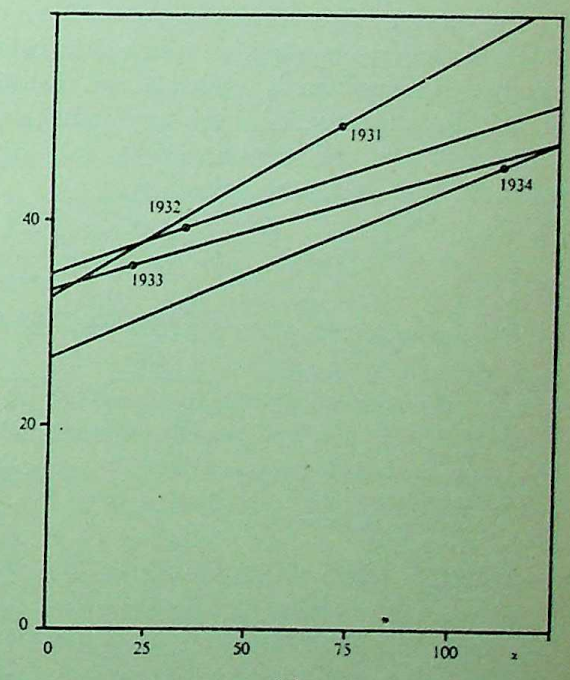

(*e*)

FIG. 4.—Point estimates of λ and α and direction of the confidence regions.

4(*a*) Austin 7.
4(*b*) Morris 12/4.
4(*c*) Alvis 12/50.
4(*d*) Talbot "75".
4(*e*) Humber 24/6 Pullman.

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Further Estimates of the Depreciation Rate

Until now our main concern has been to establish the relevance of the present model to secondhand prices. While in this respect the results have been reassuring, they have also made it clear that a straightforward application of equation (13) does not lead to satisfactory estimates of either of the single parameters λ and α. To examine the depreciation rate in different periods or for different cars on a comparable basis, at least part of the fortuitous variation related to the accompanying estimates of the scrap value must be eliminated. To do so an *a priori* value can be assigned to the latter which is then treated as a constant. Denoting this by α_0 the equation for secondhand prices becomes

$$p^*(t) = (p_0 - \alpha_0)\, e^{-\lambda t} + \alpha_0. \qquad (14)$$

This simplification suggests a change in estimation. Instead of the original observations we can consider successive differences and thus allow for the serial correlation among the error terms of the former. It is computationally simplest to consider the quotient of successive secondhand prices, corrected for the *a priori* fixed scrap value, and assume that an additive error term v applies. Or, we estimate λ from

$$\frac{p^*(t) - \alpha_0}{p^*(t-1) - \alpha_0} = e^{-\lambda} + v. \qquad (15)$$

This equation has been applied to the data used before. The value of α_0 was set at £15 for the seven cheapest car models (priced, when new, at less than £200), and at zero for the others. This corresponds roughly to the overall tendency of the estimates of α obtained earlier; its adoption rests on the belief that the genuine scrap value of a completely obsolescent car is negligible, and that the high values obtained for popular cars in the thirties in fact represent the large trade-in allowances which were then current.

Table 4 gives the resulting values of λ, together with s, the standard deviation of the residuals. The latter are defined by

$$u(t) = \{p^*(t) - \alpha_0\} - e^{-\lambda}\{p^*(t-1) - \alpha_0\}$$

TABLE 4

Estimates from Equation (15)

	1931		1932		1933		1934	
Car Type	λ	s	λ	s	λ	s	λ	s
Austin 7	·375	1·9	·311	3·7	·253	2·4	·237	2·9
Ford 8	..	..	..	..	·302	3·0	·242	2·6
Hillman Minx	..	..	·271	5·5	·298	4·5	·257	2·4
Austin 10	..	..	..	..	·274	5·4	·197	5·0
Austin 10 de Luxe	..	..	·292	3·5	·264	2·2	·260	2·9
Morris 12/4	·590	2·9	·421	3·6	·290	2·7	·416	3·2
Wolseley 12/6	..	..	·502	3·1	·477	2·1	·365	3·0
S.S. 16	..	..	·371	5·9	·348	5·6	·439	10·7
Alvis 12/50	·306	9·7	·295	4·7	·300	12·7	·250	9·4
Buick 8	..	..	·330	22·7	·415	10·8	·451	10·0
Talbot "75"	·486	17·1	·435	9·6	·389	28·6	·422	11·9
Humber 24/6	·246	14·0	·238	16·4	·345	4·5	·322	8·5
Lagonda 4·5 l.	..	..	..	..	..	..	·413	9·8
Bentley 3·5 l.	..	..	..	..	..	..	·262	76·8
Rolls Royce 40/50	·213	56·1	*		·384	121·2	*	

Note.—In four cases a single awkward observation was omitted from the calculations. Had these values been included the estimates of λ concerned would have been:

Hillman 1932	−·041	Buick 1932	−·058
Morris 1931	+·891	Humber 1932	+·180

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Although the estimation method is different, these residuals are comparable with those obtained in the earlier application; they have similarly been expressed in £. It was to be expected that the goodness of fit would suffer as a result of the advance fixing of α_0; on the other hand, however, the earlier equation had to satisfy the initial price p_0, and this constraint is now removed. It is found that on balance the residual variance has increased for most cars, though not by large amounts.

According to these estimates, the depreciation rate would vary from ·25 to ·45 with a median value of ·31. There is still considerable variation between cars and between model years which has to remain unexplained: no clear pattern is discernible among the individual estimates.

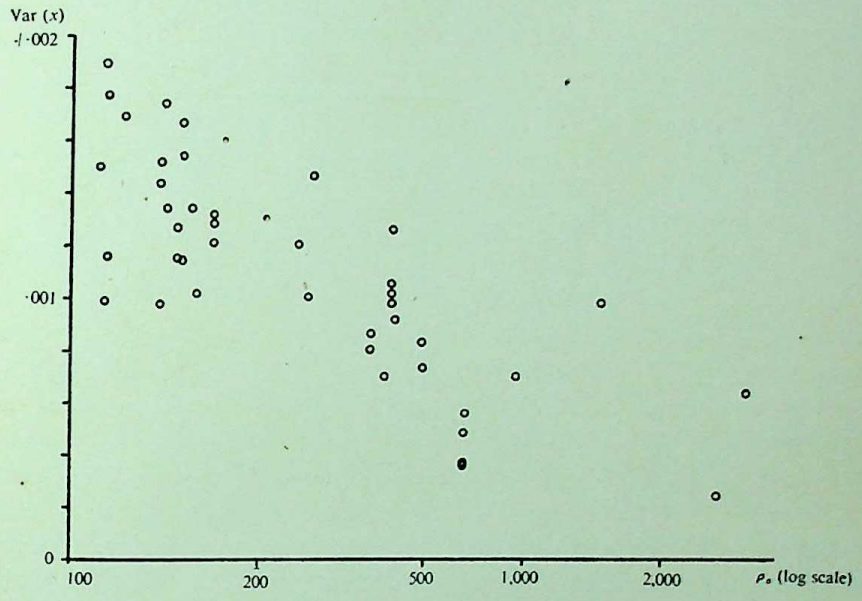

FIG. 5.—New prices and average "quantity" variance, all cars.

It should be noted in particular that there is no evidence that the depreciation rate declines for the more expensive cars; so far as it varies systematically at all there is a slight tendency for it to increase. At first sight this would appear to contradict the general opinion that high quality cars last longer and therefore depreciate more slowly. In the present model, however, a higher depreciation rate is perfectly compatible with a longer expectation of life provided there is a corresponding increase in ν. This can be seen from equation (10).

Although the price data do not permit ν to be properly estimated, the argument may superficially be checked by considering the variance of the car quantity x. According to equation (7) this varies inversely with ν, and an indication of its actual value can be found in the known secondhand price ranges. As these behave most erratically and are probably rather unreliable, we have taken the average of the variance which they suggest for a period of three years, which is the same for all cars and extends from $2\frac{1}{4}$ to $5\frac{1}{4}$ years. In Fig. 5 these averages have been plotted against the new prices of the cars concerned. There is a wide scatter, but it does show a tendency for the variance to decline which would agree with an increase in ν.

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A final set of estimates of λ has been derived for future comparison with the analysis of scrappage figures in the next section. In order to detect any variation between years they are based on cross-section data; equation (15) has been applied to the secondhand prices of successive age-groups ruling at the same date. For the United Kingdom a more or less representative selection has been made from the cars considered earlier; the United States are represented by three rival makes of the ordinary four-door saloon, the data being taken from Smith (1941); finally, the French data from Boiteux (1956) have been included. For each country α_0 was fixed at an *a priori* value thought to correspond roughly to the pre-war level of trade-in allowances.

TABLE 5

Estimates of λ from Cross-section Data

	Date of Observation			
	1936	1937	1938	1939
United Kingdom (α_0 = £15)				
Austin 7	·326	·301	·307	·295
Ford 8	·313	·303	·338	·313
Austin 10 de Luxe	·271	·283	·341	·300
Hillman Minx	·314	·303	·438	·333
Morris 12/4	·472	·441	·544	·495
Wolseley 12/6	·422	·375	·393	·488
United States (α_0 = $50)				
Chevrolet	·295	·320	·369	..
Ford V.8	·311	·343	·394	..
Plymouth	·395	·375	·433	..
France (α_0 = frs. 300)				
Renault 4	·365	·305	·293	·286

The measurement of agreement between these estimates is reassuring. As regards the variation of λ over time, the French figures are declining while those for the United States show an increase. Such impressions may well be superficial, as they are based on a few series. The inclusion of a wider range for the United Kingdom leads to confusion rather than to firmer conclusions.

4. SOME APPLICATIONS TO CAR MORTALITY

The application of the model to the mortality or wastage of motor-cars is probably of more practical importance than the analysis of secondhand prices which has just been completed. The main underlying assumption is that the passage of a car to zero quantity or value coincides with its scrapping. This provides a link with the general model of depreciation which specifies a particular form of the mortality function for cars in the same way as the hypotheses of Makeham and Gompertz were meant to do for human mortality. The coefficients of this mortality function can be estimated and are to some extent capable of a causal interpretation. A consideration of their variation may improve the consistency (if not the accuracy) of forecasts of future car scrappage.

The basic assumption just mentioned identifies the density of the quantity distribution at zero with a cumulative mortality function. It may be recalled that, as a function of age, this density is

$$P(o, t) = 1 - e^{-\mu t}\{1 - (1 - e^{-\lambda t})^{\nu}\}.$$

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The fraction of cars of some given initial generation which survives to age t is therefore given by

$$S(t) = e^{-\mu t}\{1 - (1 - e^{-\lambda t})^{\nu}\} \quad . \quad . \quad . \quad . \quad (16)$$

and this is the formula which will be considered here. To begin with, estimates of the three parameters are derived by fitting it by least squares to observed series of the fraction of cars surviving. As before, the equation does not permit direct estimators to be derived, and the iterative scoring method had to be used again; and again the actual computations were carried out by means of EDSAC I. No further adjustment was made to the estimates when the inequalities

$$|\Delta\mu| < \cdot 0001 \qquad |\Delta\lambda| < \cdot 001 \qquad |\Delta\nu| < 1$$

simultaneously obtained, both μ and λ being measured as an annual rate.

Data which give the fraction of motor-cars surviving to various ages are rare, although the information which is required must exist in the tax files of many countries. The only statistics available for the United Kingdom consist of two surveys of registrations in a few counties for the years 1936–37 and 1937–38, published in *The Motor Industry of Great Britain* for 1938 and 1939. A breakdown of registrations by the age of the car for two successive years provides specific survival rates for each annual age-group. Series of surviving fractions in terms of a given initial generation, which is the form required here, can be derived from these specific year-to-year survival rates by successive multiplication.

For the United States, tabulations of national car registrations by "model year" have been compiled by R. L. Polk and Company. They have been published in *Automobile Facts and Figures*. A minor complication in the calculation of the fraction surviving from a given generation is that the initial number of cars of the model years is not given. These data have therefore been estimated independently from new registration figures. The necessary inaccuracy of such estimates may slightly shift the overall level of the series of fractions surviving obtained, but it is not likely to affect the relation between successive observations. A second problem is the treatment of the war years. It seems obvious that the period from 1942 to 1946 should count for less than the full four years. Alternative calculations have therefore been made in which the interval was treated as one and two years respectively. The best fit was obtained by using two years for the earlier generations (up to 1934) and counting the war as one year only for the later models. This arbitrary distinction was adopted and led to a reasonably consistent set of estimates.*

Estimates for the Pre-war Period

The results of applying equation (16) to these data are set out in Table 6. As three distinct coefficients are determined for an equation with only one independent variable, a good fit may be expected; but for some of the American generations of the middle thirties, it is in fact obtained. Again, the correlation coefficients are of minor importance as any model reproducing the decline of the survivors' fraction would do well on that account. The residual standard error s is a more reliable characteristic. Moreover, there is a reassuring measure of agreement between the first differences of the calculated and observed values, as shown in Fig. 6.

* The British and American series have been reproduced in the Appendix.

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TABLE 6

Estimates for Equation (16)

	μ	λ	ν	R^2	s
United Kingdom—					
Cross-section surveys:					
1936–37 . . .	·0182 (·0018)	·542 (·017)	65 (10)	·997	·019
1937–38 . . .	·0150 (·0005)	·512 (·005)	75 (12)	·999	·006
United States—					
Model year generations:					
1930 . . .	·0225 (·0010)	·347 (·009)	37 (4)	·994	·017
1931 . . .	·0080 (·0015)	·356 (·012)	37 (5)	·990	·025
1932 . . .	·0108 (·0023)	·350 (·017)	30 (6)	·985	·033
1933 . . .	·0144 (·0028)	·296 (·017)	18 (4)	·976	·042
1934 . . .	·0103 (·0042)	·308 (·018)	14 (3)	·970	·050
1935 . . .	·0090 (·0035)	·299 (·015)	15 (3)	·978	·045
1936 . . .	·0192 (·0015)	·475 (·021)	148 (37)	·993	·025
1937 . . .	·0136 (·0006)	·465 (·007)	117 (9)	·999	·009
1938 . . .	·0042 (·0006)	·428 (·006)	64 (4)	·998	·010
1939 . . .	·0174 (·0009)	·430 (·012)	73 (10)	·995	·016
1940 . . .	·0042 (·0007)	·394 (·006)	40 (3)	·996	·013
1941 . . .	·0098 (·0010)	·430 (·020)	50 (14)	·996	·012
1942 . . .	·0155 (·0006)	·461 (·009)	73 (5)	·987	·020

The estimates of the parameters are also affected by the fact that they are determined from only one independent variable. This is bound to lead to the same interdependence of the separate point estimates which was encountered earlier. In the present case μ may be expected to be relatively unaffected; it is mainly determined by the observations for the first few years, which reflect only the accident rate, to the exclusion of the effect of ν and λ, which begins operating only at later ages. In fact, the two year-to-year surveys for the United Kingdom give very much the same result with a plausible accident rate of about 1·7 per cent. per annum. For the United States the estimates of μ vary considerably, although with a central tendency of about the same magnitude. Precisely because this parameter is mainly determined by the first few observations of each series the individual variation between generations can largely be attributed to the inaccuracies of the estimates of the initial size of each generation. This will particularly affect the earlier observations of each set.

The values of ν and λ obtained for the United Kingdom are again very similar in both years. The depreciation rate is much higher than one would expect; we will discuss its value more fully below. For the United States on the other hand, the overall level of λ is more sensible; but as we have already argued it must be considered in conjunction with ν, and this is far less well-behaved. It is tempting to disregard the low values of both parameters in 1933–35 and the very high values in 1936 and 1937, ascribing both to the unfortunate effect of small errors on their joint variation. The series of both ν and λ could then be smoothed to show a gradual increase from 1930 to 1942 which is quite plausible. On the basis of the present calculations this is an arbitrary and rather speculative adjustment; it is only slightly confirmed by the fact that the large variations of ν and λ cancel out in so far as the mean and variance of a car's life distribution are concerned.

This distribution of car lives is the frequency function corresponding to the distribution function given by the cumulative mortality; it has already been derived explicitly in equation (8). Fig. 6 shows what it looks like. During the first few years, car scrappage is due only to the operation of the accident rate; in later years there is a sharp rise as

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scrappage due to gradual depreciation is superimposed on this. Formulae for the mean of this distribution—the life expectancy of a new car—and its variance have been given earlier (equations 10 and 11). Evaluated for the estimates of subsequent American generations they are remarkably stable; the mean is between 10 and 11 years throughout the period considered, and the standard deviation is about 4·5 years. For the United Kingdom, however, the expected life of a car is only about 8 or 9 years, and the standard deviation is 3 years.

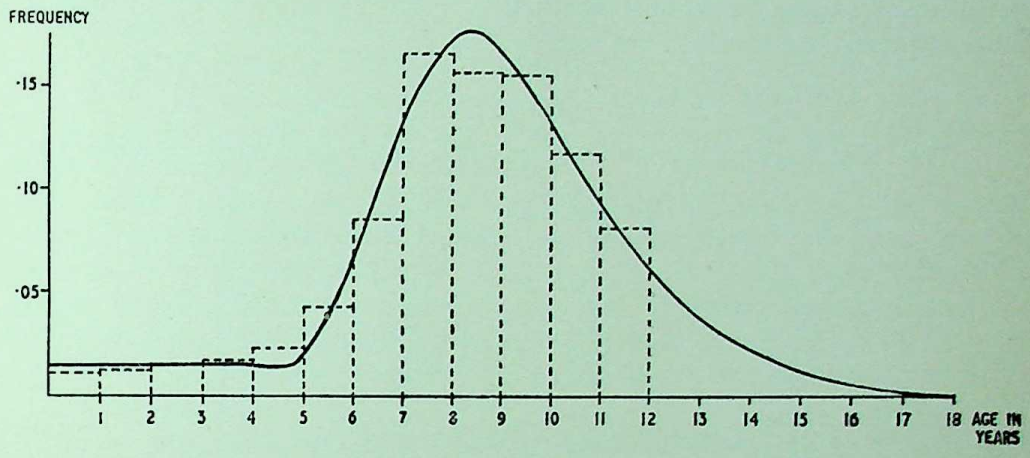

FIG. 6.—Observed and calculated life distribution of motor-cars, United Kingdom 1937–38.

For the depreciation rate λ we may compare the present estimates with those obtained earlier from secondhand prices. The relevant values are the estimates from cross-section data given in Table 5. If we accept that the variation in ν and λ among the American figures is due to their being jointly sensitive to small errors, and adopt the view expressed above, the agreement for the United States is relatively good. The depreciation rate of about ·35 which we would thus assume for the middle thirties is about the same as that derived from secondhand prices. Indeed the latter show a similar increase, as is suggested by the scrappage figures.

For the United Kingdom, however, the present estimates of λ are much higher than those found from secondhand prices. It is true that a depreciation rate of about 50 per cent. per annum is shown by the prices of the Morris Cowley, but this is an isolated instance; if we turn to the larger number of cars analysed earlier, the modal value of λ is clearly found to be ·30 rather than ·50. The only way to reconcile the two sets of estimates appears to be to assume that the mortality results are distorted by the same mechanism which we have blamed for much of the variation in the American figures. This would lead us to suppose that the "true value" of ν is lower than those obtained, and that the correct value of λ at about ·3 would follow even from the mortality data if only ν were fixed in advance at a lower *a priori* chosen value. Unfortunately there exists no extraneous evidence from which such a value of ν could be derived.

A Comparison with the Present Situation

It should not be thought that the results of the two United Kingdom surveys are unreliable or that the short life expectation they suggest did not apply. As a check, the

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observed mortality rates can be applied to successive generations of new registrations in previous years. The age-composition of the total population which is thus derived for 1938 corresponds quite well to the composition of the survey sample, and the implied scrappage agrees equally with the apparent wastage which can be deduced from the national registration figures. It appears therefore that before the war the life expectation of cars in this country was quite short.

Since the war, however, the situation has been drastically changed. Table 7 shows how apparent wastage, based on national registrations, is only a fraction of what it used to be, although the total car population has been doubled.

TABLE 7

Total Car Registrations and Apparent Wastage in the United Kingdom

Year	*Total Registrations in Third Quarter* (1)	*Apparent Wastage in Next 12 Months* (2)	(2) *As a Percentage of* (1) (3)
1934	1334	110	8·2
1935	1505	140	9·3
1936	1675	159	9·5
1937	1834	130	7·1
1938	1984	198	10·0
1951	2433	30	1·2
1952	2365	17	·7
1953	2825	29	1·0
1954	3173	60	1·9
1955	3610	87	2·4

In the early years after the war the low apparent wastage can be explained by the fact that laid-up cars were gradually being brought back into circulation. But for the last five years this factor can hardly have had any appreciable effect on registration figures. We must assume that in fact very few cars have been scrapped at all. If our model is at all applicable to these post-war years its parameters must have changed considerably; conversely, by making suitable assumptions about these changes we should be able to reproduce the apparent wastage observed during recent years and arrive at a forecast of its future course.

Any attempt to use the model in this manner must necessarily be based on rough guesses. No post-war mortality tables are available and the only parameter about which there is information is λ which may again be estimated from secondhand prices. Its

TABLE 8

Estimates of λ for Recent Years

Car Type	1951	1952	1953	1954	1955	1956	1957
Pre-war models:							
Ford 8	·180	·169	·180	·184	·205	·256	·213
Morris 8	·169	·171	·196	·190	·229	·264	·311
Hillman Minx	·161	·162	·180	·161	·182	·170	·190
Austin 10	·150	·146	·195	·177	·182	·210	·223
Post-war models:							
Ford Popular	·097	·102	·069	·062	·049	·045	·056
Morris Minor	·091	·108	·080	·065	·053	·062	·065
Hillman Minx	·122	·109	·097	·091	·066	·070	·064
Austin A.40	·089	·105	·086	·084	·075	·079	·075

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value has been calculated for four car types using cross-section secondhand price data in the manner of equation (15); the value of α_0 was put at zero, as trade-in allowances have not yet been reintroduced. To achieve uniformity between years the new price was left out of the calculations for post-war models, and the 1940 model for pre-war cars; the two groups have been treated separately. All models are saloon cars.

The estimates vary far less between years and models than those for the pre-war period, but this is probably due to the fact that the post-war data from *Glass's Guide* are based on expert opinion rather than actual observation. More interest attaches to the level of λ which is altogether much lower than before the war. Until 1955 the depreciation rate for pre-war models was only half of what it used to be, and even now it is still below its pre-war level. The value of λ for post-war years is even lower and moreover shows a steady decline so that it is now not more than 7 per cent. per annum. This compares with pre-war estimates of 30 to 40 per cent.

This sharp fall in the value of λ goes a long way to explain the decline in car scrappage, but it does not by itself suffice. An accident rate at the pre-war level of 1·5 per cent. would, even with zero depreciation rates, lead to higher wastage figures than have been observed; we must therefore conclude that μ has been somewhat affected by the changed economic conditions, and that it has declined e.g. because of a change in the amount of repairs car owners are willing to accept. It has, therefore, tentatively been put equal to 1 per cent. per annum.

As for the third parameter ν, although it has an appreciable influence on the scrappage of older cars there is not a clue as to its value in post-war years. In all subsequent calculations it has therefore been kept constant at about its pre-war value.

Before an attempt at forecasting future scrappage can be made, the course of total registrations in recent years must be reconstructed by the model in order to determine the age distribution of the present stock of cars. The actual age composition is last known for 1938 in which year the second of the surveys used earlier was made. We may apply the age-distribution of the three counties concerned to the national total of all registrations and extrapolate from this starting point by successively applying the differential mortality rates corresponding to changing values of the three parameters controlling the process.

Such an extrapolation over twenty years is, of course, highly speculative, especially as the treatment of the parameters is largely arbitrary. Various alternative procedures can be adopted but what we in fact have done is briefly as follows. For 1938–39 we have used a hypothetical pre-war mortality table with values of the parameters in between those observed in the immediately preceding years. The war years 1940–46 were treated as a period of two years' duration in which the same pre-war mortality table operated. This is analogous to our treatment of the United States' figures; one of the two war years quite probably corresponds to the actual year from September 1939 to September 1940. The radical changes in the parameters already discussed have been introduced from 1946 onwards. For all years μ was put at 1 per cent. instead of 1·7 per cent. per annum; the variation of λ up to 1951 is largely guesswork, and from there on the values suggested by the secondhand prices were used. The parameter ν was kept constant throughout at the value of 70. A complete summary of the various assumptions is given in Table 9. It should be appreciated that all post-war cars are still so new that with any value of λ smaller than, say, ·10, their present scrappage is exclusively due to the operation of the accident rate μ. It is therefore immaterial what exact value is assigned to λ within that range.

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TABLE 9

Parameters Used in Calculation of Car Scrappage in the United Kingdom, 1938–57

Year	*Pre-war Cars* μ	*Pre-war Cars* λ	*Post-war Cars* μ	*Post-war Cars* λ
1938–39 and 1940–46 taken as a two-year period	·0165	·527		
1946–47 to 1949–50		·15		
1950–51 and 1951–52		·17		
1952–53 and 1953–54	·01	·18	·01	<·10
1954–55		·20		
1955–56 and 1956–57				

The parameter was assumed to be 70 in all cases.

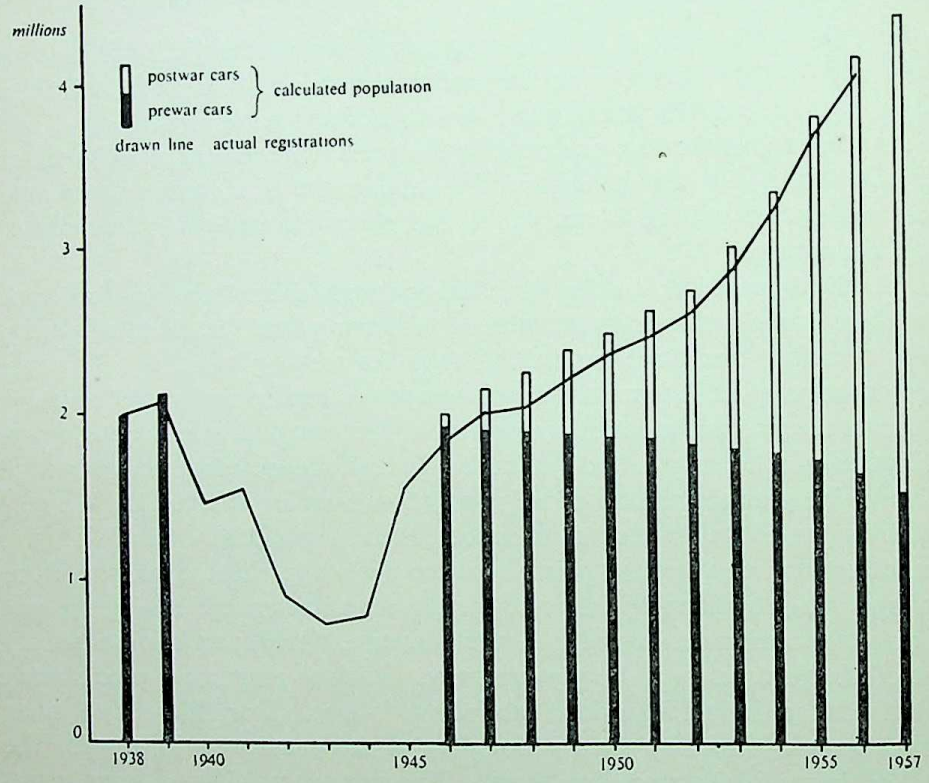

FIG. 7.—Calculated car population and actual registrations, United Kingdom 1938–57.

By successive application of these mortality functions and known new registrations to the 1938 figures the development of the car population since that year can be reconstructed. The result is shown in Fig. 7. Although there is a persistant discrepancy of about a hundred thousand cars between the actual observed registrations and the calculated population the overall agreement is satisfactory. But this again is to a large extent because both the actual and the calculated series have the same new registrations in common, and in the absence of considerable scrappage this is the main determinant of year-to-year changes. A more stringent test of the model is a comparison of the calculated and observed apparent wastage of cars implied by the series of Fig. 7.

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TABLE 10

Actual and Calculated Car Scrappage, United Kingdom, 1950–57

	Year Ending Third Quarter of							*Thousands*
	1950	1951	1952	1953	1954	1955	1956	1957
Calculated scrappage .	21	29	25	35	37	62	113	140
Apparent wastage .	8	18	30	17	29	60	87	..

The very low apparent wastage in 1950 may be due to re-registration of old cars which had hitherto been laid up; in preceding years this factor often leads to negative apparent wastage being shown by the registration figures. For the years since 1951 the calculated scrappage approximates actual apparent wastage as well as might be expected for an extrapolation of the extent involved. It is particularly notable that the increase in apparent wastage in 1955 and 1956 can be explained satisfactorily by the operation of the higher values of λ for pre-war cars suggested by secondhand prices.

Finally, these calculations give an estimate of the percentage age distribution of the car population in recent years. The figures for 1957 have been applied to an estimated total of 4·33 million cars in the third quarter of that year and it is this age composition, reproduced in Table 11, which serves as a starting point of a forecast of future scrappage.

TABLE 11

Estimated Car Population in the United Kingdom, Third Quarter 1957, by Age

Year of First Registration	*Thousands Number in 1957*
1957 .	400
1956 .	456
1955 .	487
1954 .	366
1953 .	266
1952 .	154
1951 .	135
1950 .	128
1949 .	135
1948 .	101
1947 .	153
1946 .	75
1940–45 .	46
1939 .	247
1938 .	216
1937 .	239
1936 .	227
1935 .	186
1934 .	121
1933 .	75
1932 .	44
1931 .	29
1930 .	20
Before 1930 .	24
Total .	4330

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The figures in this table are given in thousands and by annual age-groups in order to permit their use in any required grouping; but of course no accuracy of this order can be claimed for them. The major proportions, however, should be correct; pre-war cars, for example, should in fact account for 35 per cent. of the total car population.

This estimate of the present age composition of the car population may now be used for a tentative forecast of the future course of car scrappage. Such a forecast is of course conditional upon the validity of its underlying assumptions, viz. in the present case the hypothesis that present conditions continue without further change. It is therefore implied that the parameters alleged to control car scrappage remain fixed at the values obtaining in 1956 and 1957. It is of course unlikely that this will in fact be so, but this assumption is clearer and less hazardous than most others.

No assumption will be made about the growth in the total car population by the addition of new cars. So long as the values of λ and ν for post-war cars remain at all near the present figures these new cars will contribute to total scrappage only through the operation of the accident rate. Now the effect of the latter is that of a flat percentage of the total car population; we may therefore leave it out of account altogether and forecast future scrappage as about 1 per cent. of the total population *plus* a specific component which represents the gradual obsolescence of the pre-war stock. For this latter component the following figures are obtained:

TABLE 12

Estimated Scrappage Due to Obsolescence of Pre-war Cars, 1957–67

Year	*Estimated Scrappage*	*Year*	*Thousands Estimated Scrappage*
1957	90	1962	120
1958	110	1963	110
1959	110	1964	100
1960	120	1965	90
1961	120	1966	70

The figures are given to the nearest ten thousand and they show the general implication of present trends rather than constitute an accurate forecast. Still they illustrate well how the present year only shows the beginning of the end of pre-war cars. This, incidentally, may account for the striking recovery of car sales in recent months. But, as the figures show, a steady and gradual scrappage of old cars at a slightly higher level may be expected during the coming years.

It should be emphasized that these figures represent only scrappage of pre-war cars. It is tempting to speculate on the date when the post-war generations will first begin to add to scrappage through obsolescence. At present values of the parameters, however, this date is so improbably distant that it is most likely that in fact the parameters will change and lead to an earlier scrappage. The figures given are, therefore, a minimum which is likely to be surpassed.

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APPENDIX

Motor-Car Mortality Data

The basic data for the United Kingdom consist of two surveys of car registrations in selected regions. Each provides tabulations of total registrations according to year of first registration for two successive years. The figures at the later date of each pair have been proportionately reduced to allow for population increases. These corrected figures, reproduced below, have been taken from *The Motor Industry of Great Britain*, 1938 and 1939.

The quotient of the numbers of surviving cars at successive dates gives the specific survival rate for each age-group. Successive multiplication of these specific survival rates produces the fractions set out in Table A3.

TABLE A1

Basic Data for the United Kingdom

Year of First Registration	*Middlesex*		*Middlesex, Norfolk and Lancashire*	
	September 1936	*September* 1937	*September* 1937	*September* 1938
1938	..	..	..	26333*
1937	..	18731*	30347*	30023
1936	18578*	18507	30265	29855
1935	16154	16041	27331	26927
1934	12592	12329	21596	21230
1933	9979	9658	17531	17103
1932	7904	7424	13981	13328
1931	6782	6211	11845	10695
1930	7595	6418	11551	9143
1929	7096	5413	9808	7349
1928	5210	3432	6088	4079
1927	3570	2065	3700	2310
1926	1940	972	1756	1023
1925 and earlier	1905	867	1667	986
Unknown	6	6	..	..
Total	99311	108074	187466	200384

* September registrations plus sales during the last quarter of the year.

The basic data for the United States consist of specifications of the total national registrations by "model year" which have been compiled annually since 1935 by R. L. Polk and Company. These figures have been published in *Automobile Facts and Figures* and the 1946–47 and 1956 editions together contain the data for all years from 1935 to 1955.

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TABLE A2

Basic Data for the United States

Model Year	*Initial Size of Generation*	*National Car Registrations per July 1st* ×1000																
		1935	1936	1937	1938	1939	1940	1941	1946	1947	1948	1949	1950	1951	1952	1953	1954	1955
1930	2626	2362	2288	2164	1914	1738	1484	1205	592	..	..	..	..	..	..	..	..	..
1931	1908	1867	1851	1816	1682	1610	1438	1211	626	596	..	..	..	..	..	..	..	..
1932	1096	1050	1051	1041	975	954	865	735	369	346	317	..	..	..	..	..	..	..
1933	1494	1420	1422	1409	1333	1326	1250	1120	607	568	509	435	..	..	..	..	..	..
1934	1889	1807	1825	1824	1759	1759	1697	1591	1021	967	886	762	567	..	207	..	..	..
1935	2340	..	2378	2351	2261	2258	2220	2139	1675	1592	1490	1335	1053	688	430	294	..	..
1936	3345	..	..	3324	3262	3261	3234	3175	2893	2790	2657	2495	2074	1493	996	698	444	..
1937	3614	..	..	..	3536	3532	3499	3481	3390	3304	3186	3075	2709	2126	1541	1130	746	481
1938	1735	..	..	..	..	1739	1746	1733	1736	1679	1662	1621	1490	1244	973	752	517	344
1939	2560	..	..	..	..	..	2423	2402	2426	2379	2345	2326	2244	1996	1687	1396	1015	693
1940	3145	..	..	..	..	..	..	3112	3182	3113	3093	3089	3033	2794	2496	2190	1691	1205
1941	4130	..	..	..	..	..	..	..	4107	4086	4004	4005	3955	3761	3456	3095	2462	1782
1942	1071	..	..	..	..	..	..	..	997	1065	1012	1006	999	966	892	799	640	464

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TABLE A3

Fractions of Cars Surviving Used in the Estimates of Table 6

Age in Years	*United Kingdom Surveys*		*United States Model Year Generations*												
	1936–37	1937–38	1930	1931	1932	1933	1934	1935	1936	1937	1938	1939	1940	1941	1942
1	·996	·989	..	..	..	..	·957	1·016	·994	·978	1·002	·946	·990	·994	·931
2	·989	·976	..	..	..	·950	·966	1·005	·975	·977	1·006	·938	1·012	·989	·994
3	·969	·962	..	..	·958	·952	·966	·966	·975	·968	·999	·948	·990	·969	·945
4	·937	·945	..	·979	·959	·943	·931	·965	·967	·963	1·001	·929	·983	·970	·939
5	·881	·922	·899	·970	·950	·892	·931	·949	·949	·938	·968	·916	·982	·958	·933
6	·806	·879	·871	·952	·890	·888	·898	·914	·865	·914	·958	·909	·964	·911	·902
7	·681	·794	·824	·882	·870	·837	·842	·716	·834	·882	·934	·877	·888	·837	·833
8	·520	·628	·729	·844	·789	·750	·540	·680	·794	·851	·859	·740	·794	·749	·746
9	·342	·471	·662	·754	·671	..	·512	·637	·746	·750	·717	·659	·696	·596	·598
10	·198	·315	·565	·635	..	·406	·469	·571	·620	·588	·561	·545	·538	·431	·433
11	·099	·197	·459	..	·337	·380	·403	·450	·446	·426	·433	·396	·383	..	..
12	..	·115	..	·328	·316	·341	·300	·294	·298	·313	·298	·271	..	..	..
13	..	..	·225	·312	·289	·291	..	·184	·209	·206	·198	..	..	..	..
14	..	..	..	..	..	..	·110	·126	·133	·133	..	..	..	..	..
15	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..

Bars indicate war years.

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The initial size of each "model year" generation is, however, not directly given. The figures in Table A2 for this item are:

1930–34.—R. L. Polk annual registration figures as reproduced by Smith (1942).

1935–42.—R. L. Polk figures of current model registrations on July 1st of the year concerned *plus* next twelve months' registrations *minus* the number of current model registrations at the later date.

The latter registration figures have also been taken from *Automobile Facts and Figures*.

The fractions in Table A3 are the quotient of the number of surviving cars of a given model year and the initial size of that model year generation. For the treatment of the period 1941–46, see main text.

Discussion on Mr. Cramer's Paper

Professor Stone: I am very pleased to be given the opportunity of proposing a vote of thanks to Mr. Cramer for his most interesting paper. His subject is an important one and it is good to see it studied as a statistical phenomenon amenable to empirical examination. Accountants and economists alike are concerned with the problem of how the gradual wearing out of durable goods should be reflected in private and national accounts and will welcome attempts, such as Mr. Cramer has made, to show by detailed study the means by which an appropriate formulation of the phenomenon of depreciation can be made.

I find Mr. Cramer's paper particularly interesting because I have been working for some time on the demand for durable goods and find, in doing so, that it is necessary to introduce a concept of consumption, which I think of as depreciation, as well as the more straightforward concept of purchases. In order to have a definite model for demand it is necessary to define the depreciation process and estimate its parameters. Partly because of its favour among accountants in this country and partly because it fits comfortably into the formulation of my demand model I have adopted the reducing balance method of estimating depreciation which in a sense is the deterministic analogue of Mr. Cramer's stochastic formulation.

Mr. Cramer does not stop at a theoretical formulation but tries by various means to estimate the parameters in the model of depreciation. This is also important for the demand analyst. For different periods Mr. Cramer arrives at estimates which, if true, are useful, and somewhat different from what might have been expected. They reveal however a complication, namely, that there has been a large apparent fall in the depreciation rate for motor-cars since before the war. This means that the assumption of a constant depreciation rate over time may be seriously misleading and not merely a minor source of inaccuracy. I hope we shall hear this evening from those with practical knowledge of the motor trade what they think of Mr. Cramer's estimates and of the large change which they suggest has taken place. I wonder, for example, how far the apparently large depreciation rates relating to the pre-war period can be taken to reflect a high degree of obsolescence in a rapidly developing product and how far the low rates of the post-war period reflect a much higher degree of technical stability in which obsolescence is relatively unimportant.

I hope that this paper will stimulate further the intensive study of depreciation. The author and the Society may well feel proud of it and I have great pleasure in proposing a hearty vote of thanks to Mr. Cramer.

Sir Robert Shone (seconding the vote of thanks): I am delighted to have the opportunity of speaking on Mr. Cramer's paper, as depreciation is one of the matters on the border line of economics and accounting which, in spite of its great importance, has not been effectively analysed. A more scientific assessment of the magnitude of depreciation

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and of the factors which determine it would be of immense value to industries and firms as well as in assessing the national economic position.

Mr. Cramer's main point appears to be that his theory of chance mortality gives a reducing balance or exponential form of depreciation. Thus, if the chance of dying is always 10 per cent. in a given year then we get a mortality or depreciation curve of 10 per cent. per annum.

If I have interpreted Mr. Cramer correctly, I am glad to be able to say that on his main premise I am one of his supporters, at least to the extent of believing that the frequently used straight line depreciation does not give an approximately correct description of the course of depreciation in many of the instances where it is used; and that some formula which gives higher depreciation in the early years, and lower in the later, more nearly fits the facts in most cases.

Where I part company with Mr. Cramer is in his basic reason for arriving at the reducing balance basis of depreciation. His basic physical assumption is one of wearing out, or the mortality of the components of a car. But I think in approaching depreciation for plant generally, perhaps more than for cars specifically, it is increasingly irrelevant to think in terms of a wearing out. I would, to put the point in its extreme form, rather think in terms of cars or industrial plant being substantially everlasting: and that first the depreciation, in value and then the ultimate replacement, result mainly from progress—progress in inventing new and cheaper methods of production, progress in quality, in design, and in cost reduction of all sorts. This leaves the old model high and dry, and ultimately a point comes at which it is uneconomic and is scrapped—not because its parts have all decayed, but because something better has been born.

In broad terms the shape of the depreciation curve one gets from a theory of progress is not very different from the one Mr. Cramer gets from a theory of decay. To illustrate the point, any concept of progress, such as doubling the standard of living in 25 years, can be taken as meaning that wage rates are likely to rise by some percentage per annum if prices are constant, or alternatively, if wages are constant, that prices can be expected to fall.

To take the latter approach first, we may assume that inventiveness, call it what you will, brings down price levels by some constant percentage per annum. Thus, if we consider the whole economy as made up of a number of major units of plant of different ages (the more modern plant being the more efficient); and that we have only two cost elements, wage rates and capital charges; then we would find the amount available for depreciation and return on capital at any given plant would tend to fall each year. Or put another way, each firm through its lifetime will be faced with an exponentially falling price curve and a constant labour cost curve. This will leave something close to an exponentially falling amount available for depreciation charges. This gives a curve much closer in shape to Mr. Cramer's curve than the frequently used straight line depreciation curve.

In the case of a car, it may be that the inventiveness takes the form of an improved model selling at the same price. If this improvement is, say, 20 per cent. per annum (a considerable part of this may be a subjective styling improvement), we may then expect at the end of a year the old model to have fallen by 20 per cent. in price, assuming its physical and other conditions are still the same. Equally, so long as this rate of progress is steadily achieved, each older model down the line also falls by 20 per cent. in relation to the previous year's model. This type of thinking again would give an exponential depreciation curve. There are, of course, undoubtedly many other factors impinging. There is the snob value of having a new as against a secondhand car which may augment significantly the degree of depreciation in the first year. There are all the vagaries of a fashion trade which complicate the matter as compared with the more materialistic considerations of depreciation that relate to an industrial plant.

I have been arguing that depreciation based on the birth of new ideas and not the death of old atoms can give something like an exponential depreciation rate, yet if a slightly different assumption is made about the way that progress is reflected economically, a

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different depreciation curve is obtained. If, for example, progress is reflected in increasing wages at constant prices, then the depreciation curve is not even roughly exponential.

The situation facing a typical plant once installed would then be a constant price but a wage cost continuously rising at a compound rate until there came a time at which there was no sum available for return on capital either in the form of depreciation or profit, and the plant would then be scrapped. The shape of the capital return curve would then be concave downwards and the depreciation curve must necessarily be roughly of the same shape. This is a curve which is close neither to a straight line nor an exponential curve. It starts at a high and fairly steady level and then falls more rapidly in the later years of life. Like the exponential curve, however, it does still have the characteristic of higher depreciation in the early years than in the later years.

Some rough calculations made with the help of Mr. Fisher on this subject have tended to suggest that if depreciation is primarily caused by the progress factors I have instanced, then under constant price and rising wage conditions, the initial rate of depreciation should be very roughly one and a half times the depreciation as calculated by the straight line method. On the other hand, the exponential method of depreciation normally gives a rate about twice that of the corresponding straight line rate.

I fully recognise that my argument has been greatly oversimplified and depreciation cannot be attributed to any one factor. Any satisfactory theory must leave room for the many different causes of depreciation or obsolescence.

I should like to thank Mr. Cramer for raising such an important question, and for working out so clearly one theory of depreciation. I hope this will stimulate further discussion and clarification of the subject, and indeed I was particularly glad that Dr. Barna in his recent paper said he was intending to give special attention to depreciation.

The vote of thanks was put to the meeting and carried unanimously.

Mr. F. J. LLOYD: My feelings on reading this paper were those of a man who although at one time was a backroom boy concerned with statistics is now engaged in bus operation. It is the connection between buses and cars which encourages me to speak.

To me, the essence of the paper appears in Table 12, which forecasts future scrappage. I acknowledge the debt we owe to our author, because to make any forecast is extremely difficult. He has had to make various assumptions, and at the beginning of his paper these assumptions are clearly set out, and we know precisely the bases of his results. I appreciate the formidable amount of arithmetic in moving from paragraph 1 to Table 12.

Unfortunately, I am not entirely happy with the forecast he has made. I am not an economist and do not entirely regard motor-cars as durable consumer goods. I prefer to regard them as a capital investment which buys mileage. I think that to-day 80 or 90 per cent. of the new motor-cars purchased are bought by or on behalf of firms primarily to provide mileage for their employees. Therefore I am inclined to put my emphasis on mileage.

The author states that approximately 35 per cent. of the cars licensed today are pre-war. I accept this figure, but I would say immediately that nothing like 35 per cent. of the mileage run today is by pre-war cars. If you lived, as I do, in a suburb where the main road goes through towards London and stood watching the traffic in the morning you would find less than one in twenty of the cars passing have pre-war radiators. That position is not surprising. Obviously a car tends to wear out, and the mileage it runs each year decreases as its age advances.

One object of this analysis is to estimate the number of new cars required to replace scrappage. I consider that the number of new cars required is much more a feature of the mileage required in a particular year, than the number of cars that are licensed at a particular time. I would illustrate this theory from my own experience. My firm previously bought new staff cars about every five years and maintained them during this period. Two or three years ago our accountants and engineers found it was more economical to buy new cars each year and to trade them in on a guaranteed basis at the end of the year. Consequently we are now buying five times as many new cars as we were a few years ago. Such schemes

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must have a great effect on the number of new cars manufacturers produce. Whether it is more economical to exchange a car after one year is a function of the cost of maintenance and the secondhand value obtainable at the end of the year. I imagine that if this policy were generally accepted secondhand values would fall. If they fell sufficiently presumably we might have to hold on to our cars for eighteen months or two years. That would immediately affect the number of new cars required on the market, although it would not necessarily affect the scrappage rate of old cars. I understand that in America there are "car cemeteries" where, because this business of exchange has become more developed, cars are scrapped very much more rapidly. That is the type of situation we may move towards in this country. My suggestion would be that in looking at the forecasts in this paper we should bear in mind two factors. First, since the war there has been a marked increase in the demand for cars in the United Kingdom and secondly, the basic requirement is to buy mileage rather than motor-cars.

Mr. H. R. FISHER (amplified in writing): Mr. Cramer's paper brings out some extremely useful data and provocative speculations on a subject which is of great practical importance to many of us—the demand for motor-cars; and for a paper with a mathematical background I found it exceptionally clear and easy to follow. The paper does, however, raise one or two points of interrogation.

There is set up a model in which a car decays rather like a packet of a few score identical radio-active atoms; indeed one is tempted to say "This is nuclear chemistry, not economics." From this model Mr. Cramer derives exponential decay of average individual car quantity, and so, by a further assumption, exponential decay of price. Such a law of price decay is a plausible and simple hypothesis, and one which one would wish to test. Whether the observations do confirm it is a question on which I would like to say something later. But whether they confirm it or not they do give us the interesting coefficients in Table 5 and Table 8, where $e^{-\lambda}$ is the average proportion by which a secondhand car's price is diminished in comparison with a similar car a year older.

From the assumption that a car goes on (failing accidents) until its last "atom" decays, and then stops, there is derived a theoretical life distribution curve of the kind shown in Diagram 6. This curve is obviously of the right general shape; and with three adjustable parameters, λ, μ, ν, one would expect to find it capable of reproducing the three main features of any ordinary mortality data—the position of the peak, the breadth of the peak, and the early level of mortality due mainly to accidents. Thus we have a useful analytical tool for comparing different sets of data, especially when, as is the case in Fig. 6, a set of data covers only part of the range.

So long as price studies and mortality studies are kept separate, the stochastic model provides a convenient and suggestive mathematical framework. Where I find it unconvincing is as a link between the two. Unfortunately, as Mr. Cramer has stressed, the paper's culminating passage on post-war retirements of pre-war cars depends vitally on a quantitative interpretation of the link between the price data and the mortality data. He shows in Table 8 that for cars of pre-war make λ, the cheapness-for-age coefficient, was in the early 1950's very much lower than before the war, but has lately been returning towards its pre-war level. Because of this, in order to work out recent past and near future retirements of pre-war cars, Mr. Cramer inserts in his decay model a similarly changing mortality-coefficient λ. In fact in Table 9 he virtually reproduces the λ's of Table 8 for post-war years.

One may doubt whether the stochastic approach is right; but let us suppose it is. I think there is still no obvious unique form of stochastic model, and I had the curiosity to work out the effect of slight variations on Mr. Cramer's model. He assumes a car is retired when its last "atom" decays. But we might with equal plausibility retire the car when the last unit but one decays—when the car is reduced to a strength of one unit; perhaps it needs that last unit to limp to the scrap yard. Or we might make the limit two units, or any other number. The consequences of such changes turn out to be rather large. I have set out in the following table (p. 52) certain characteristics of the life distribution.

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These are the formulae equivalent to Mr. Cramer's equations 10 and 11 in the case $\mu = 0$. I think the formula retains some validity for moderate μ, but we cannot speak very usefully of the standard deviation when $\mu \neq 0$ because, as may be seen from Fig. 6, the contribution of the peak to the variance is liable to be strongly supplemented or even swamped by that of the initial mortality, so one has to speak either rather vaguely of the breadth of the peak, or more exactly of the standard deviation of the associated curve in which $\mu = 0$.

Column (1) shows that in Mr. Cramer's life distribution the breadth of peak is closely related to λ and not very much to ν, or, to put it the other way round, the mortality-λ is determined within close limits by the breadth of the peak. If we neglect the small fraction $1/(\nu + \frac{1}{2})$, we may say the breadth of the peak is equal in Mr. Cramer's case to $1 \cdot 28$ times $1/\lambda$, the difference in age that reduces the price by a factor of $1/e$. From column (2) it appears that with a slight difference in the model this linking coefficient is only about $0 \cdot 8$, and if you take the third variant, where the critical number of elements is 2, the coefficient falls to about $\cdot 6$. It continues to fall, as the last column shows, as the critical number p increases.

The difference in the shape of the theoretical life distribution curves in the different cases may be traced via the third or fourth moments or cumulants, for which there are simple formulae continuing those in the last column. The third moment or cumulant is $\lambda^{-3}\, 2!\, \{S_3(\nu) - S_3(p)\}$ and the fourth cumulant is $\lambda^{-4}\, 3!\, \{S_4(\nu) - S_4(p)\}$. These give, when use is made of the asymptotic expansions of S_3 and S_4 the measures of skewness and kurtosis indicated in lines 8 and 9 of the table. Here the differences between the columns are, perhaps, a little greater than one might expect, but it seems doubtful whether the mortality data can be adequate to favour one model rather than another. Unless they do, there seems nothing to choose between these models, so a belief in stochastic decay is not sufficient to establish any unique link between price-ratios and the mortality process. Before placing faith in any particular stochastic model that links the two, should we not satisfy ourselves that it has some real objective mechanical basis, or that the implied price-mortality link is repeatedly verified in practice, and preferably both? Unfortunately, the only available data for the United Kingdom do not verify the link in the model Mr. Cramer adopts. There is a price-λ of about $0 \cdot 33$ in Table 5, based on the pre-war cross-section data, and a mortality-λ averaging $0 \cdot 527$ in Table 6. Mr. Cramer points out the difference between them. In the lower part of my table are shown the λ and ν needed in the alternative models to produce a certain pair of values of mean and variance when $\mu = 0$. I have chosen the values $9 \cdot 170$ and $5 \cdot 918$ because they correspond in Mr. Cramer's model with the $\lambda = \cdot 527$ and $\nu = 70$ adopted in Table 4 as representative of pre-war conditions on the strength of Table 6. The second model, to give the same mean and variance, requires $\lambda = 0 \cdot 3212$ and $\nu = 28$–29. Thus the pre-war agreement between the price-λ and the mortality-λ would be better—in fact as good as could be—in the second model ($p = 1$). On the other hand, if one adopted that model for post-war calculations and inserted in Table 9 for 1938–46 the modified λ just found, leaving unaltered the post-war values of λ estimated from price data, the following rough argument shows that one would get an earlier disappearance of pre-war cars in the final estimates. The mode of the mortality distribution comes, it may be shown, at the time when decay at proportionate rate λ per unit time would reduce the number of units in the car to $p + 1$. Thus for a car of effective age x years at 1946, the modal dates, which we may denote by t_0 on the supposition $p = 0$, and t_1 on the supposition $p = 1$, are given respectively by

$$\log_e 70 - \log_e 1 = \cdot 527\, x + \int_{1946}^{t_0} \lambda\, dt$$

and

$$\log_e 28 \cdot 53 - \log_e 2 = \cdot 3212\, x + \int_{1946}^{t_1} \lambda\, dt$$

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that is, on subtraction,

$$\int_{t_1}^{t_0} \lambda \, dt = 1 \cdot 5906 - \cdot 2058 \, x$$

or, since λ is taken as about $\cdot 22$ over the relevant range,

$$t_0 - t_1 = 7 \cdot 2 - 0 \cdot 94 \, x.$$

Thus t_1 is appreciably less than t_0 for the predominant values of x, say 2–6, and this would worsen the discrepancy between actual and calculated scrappage in Table 10. Thus the model $p = 1$, which pre-war unites the price-λ and mortality-λ (and incidentally, if adopted for the United Kingdom alone, would give a tempting explanation of the longer life of American cars—that coming into the hands of wage-earner final owners they were habitually taken one stage nearer decrepitude than British cars) fails to describe the post-war data. Even if we suppose a transition to $p = 0$ after the war, and so omit $\log_e 2$ in the equation above, we still get

$$t_0 - t_1 = 4 \cdot 1 - 0 \cdot 94 \, x$$

so with $x = 2 - 6$ the resultant mortality would be not greatly different from that calculated by Mr. Cramer—i.e. still too great. The fact is, I suspect, that the post-war shortage of new cars and the potential mass demand, American fashion, for old ones, has worked out not only as though p had gone back to 0, but as though ν had increased also —i.e. old cars had become endowed with extra lives.

But this is stretching stochastic decay and mortality models to breaking point. One cannot but find them fascinating, but they are not much use if they have to be supported by *ad hoc* and unpredictable changes from time to time in all 4 parameters (λ, μ, ν, p). I feel that what mainly determines price differences and retirements is much less the physical decay of cars than the state of the market—the state of consumer incomes, the availability or non-availability of new cars (which has been very important at times since the war), improvements in new models, the fact that mere newness is more attractive to some purchasers than others—not to mention the price of petrol, purchase tax and hire-purchase conditions. In the early 1950's there was a shortage of new cars and that is, I suggest, the reason why, as Table 8 shows, there was only a moderate price discrimination for age; for pre-war cars differed little in real wear or lack of modernity. To-day, with easier new car supply, the number of people who must have a car at any price does not supply an undiscriminating demand for pre-war models as it did in 1948. One can trace a similar influence of economics in Table 5. The values of λ there show that price discrimination was stronger in 1938 than in 1937 or 1939. We know that 1938 was comparatively a depression year. Might it be—it is only a surmise—that the depression affected at one end the business owners, who could easily save cash by moderating their normal habit of buying new cars and selling one-or-two-year-old cars, and at the other end the wage-earning and unemployment-conscious potential buyers of old cars, the middle-class private car owner being left untroubled? A reduction in supply of one-or-two-year-old cars and a reduction in demand for eight-or-nine-year-old cars would accentuate the price difference for age.

This insistence on economics is not to deny that there is a link between the cheapening-for-age coefficient and the tendency to mortality. Obviously such a link exists, and it is in the same direction as the stochastic models suggest, but I doubt whether such models can be brought in aid to quantify it. The fact that price discrimination between pre-war cars is gaining strength is evidence that these cars will soon be at last on their way out. It is very useful to know this and we must be grateful for having it put on record.

Finally, a word on the hypothesis of exponential decay with age. I do not think Mr. Cramer would claim to have verified this form of decay against other possibilities, for example Boiteux's empirical law. Mr. Cramer is precluded from quoting his data in detail, but examination of his parameters raises suspicions of some kind of systematic departure from exponential decay. For the British market there is an interesting feature

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Life Distribution Characteristics in Case of Random Decay of Cars Consisting of ν Independent Elements Each with Liability λ dt to Fail in Interval dt

Note: $S_1(\nu) = 1 + \frac{1}{2} + \frac{1}{3} \ \ldots \ + \frac{1}{\nu}$, $S_2(\nu) = 1 + \frac{1}{2^2} + \frac{1}{3^3} \ \ldots \ + \frac{1}{\nu^2}$ etc. $S_r(0) = 0$

	Car is Scrapped when Number of Intact Elements Falls to			
	0 (*Mr. Cramer's Case*)	1	2	p
	(1)	(2)	(3)	(4)
1. Expected fraction of original elements surviving at time t ($t \ll m_1$)	$e^{-\lambda t}$	$e^{-\lambda t}$	$e^{-\lambda t}$	$e^{-\lambda t}$
2. Mean car life $= m_1 = \kappa_1$ . .	$\lambda^{-1} S_1(\nu)$	$\lambda^{-1}\{S_1(\nu) - 1\}$	$\lambda^{-1}\{S_1(\nu) - 1 - \frac{1}{2}\}$	$\lambda^{-1}\{S_1(\nu) - S_1(p)\}$
3. that is, approximately . .	$\lambda^{-1}(\log \overline{\nu+\frac{1}{2}} + 0{\cdot}5772)$	$\lambda^{-1}(\log \overline{\nu+\frac{1}{2}} - 0{\cdot}4228)$	$\lambda^{-1}(\log \overline{\nu+\frac{1}{2}} - 0{\cdot}9228)$	$\lambda^{-1}(\log \overline{\nu+\frac{1}{2}} - \log \overline{p+\frac{1}{2}})$
4. Variance of life $= m_2 = \kappa_2$ . .	$\lambda^{-2} S_2(\nu)$	$\lambda^{-2}\{S_2(\nu) - 1\}$	$\lambda^{-2}\{S_2(\nu) - 1 - \frac{1}{4}\}$	$\lambda^{-2}\{S_2(\nu) - S_2(p)\}$
5. that is, approximately . .	$\lambda^{-2}\left(1{\cdot}64493 - \frac{1}{\nu+\frac{1}{2}}\right)$	$\lambda^{-2}\left(0{\cdot}64493 - \frac{1}{\nu+\frac{1}{2}}\right)$	$\lambda^{-2}\left(0{\cdot}39493 - \frac{1}{\nu+\frac{1}{2}}\right)$	$\lambda^{-2}\left(\frac{1}{p+\frac{1}{2}} - \frac{1}{\nu+\frac{1}{2}}\right)$
6. Therefore standard distribution of life-distribution ($\mu=0$) ÷ difference in age that reduces price by factor $1/e$ = approximately . . .	$\sqrt{1{\cdot}64493 - \frac{1}{\nu+\frac{1}{2}}}$	$\sqrt{0{\cdot}64493 - \frac{1}{\nu+\frac{1}{2}}}$	$\sqrt{0{\cdot}39493 - \frac{1}{\nu+\frac{1}{2}}}$	
7. or	$1{\cdot}2825 - \frac{0{\cdot}3898}{\nu+\frac{1}{2}}$	$0{\cdot}8031 - \frac{0{\cdot}6226}{\nu+\frac{1}{2}}$	$0{\cdot}6284 - \frac{0{\cdot}7956}{\nu+\frac{1}{2}}$	$(p+\frac{1}{2})^{-\frac{1}{2}} - \frac{\frac{1}{2}(p+\frac{1}{2})^{\frac{1}{2}}}{\nu+\frac{1}{2}}$
8. Measures of skewness and kurtosis: $\gamma_1 = \sqrt{\beta_1} = \kappa_3/\kappa_2^{3/2}$ = approximately	$1{\cdot}1395 + \frac{1{\cdot}0391}{\nu + {\cdot}1962}$	$0{\cdot}7802 + \frac{1{\cdot}8147}{\nu - {\cdot}3742}$	$0{\cdot}6209 + \frac{2{\cdot}3584}{\nu - {\cdot}9567}$	
9. $\gamma_2 = \beta_2 - 3 = \kappa_4/\kappa_2^2$ = approximately .	$2{\cdot}4 + \frac{2{\cdot}9180}{\nu - {\cdot}4119}$	$1{\cdot}1875 + \frac{3{\cdot}6826}{\nu - 1{\cdot}8258}$	$0{\cdot}7626 + \frac{3{\cdot}8617}{\nu - 3{\cdot}2981}$	
10. Values of parameters which (with $\mu=0$) make Mean = 9·170, Variance = 5·918 (σ = 2·433) { λ =	0·527	0·3212	0·2441	
{ ν =	70	28·53	23·10	

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of the coefficients α and λ in Table 2. If in that table one compares the coefficients for any one particular make—and there are usually from two to four coefficients for the most popular makes—one is struck by the fact that the residual value α tends to work out higher for the later models. Of course, λ varies in the same direction for reasons explained in the text. This movement of the parameters could be due to a combination of two things; first it might be that the proportionate decline per annum gets less as the car gets older, but follows substantially the same curve for models of all years; and secondly (as is certainly true) the fitting of the λ, α formula had to be effected with a shorter record of prices for the later models.

How the mere shortening of the available record can give a higher value of α and λ can be exemplified from the data recorded by M. Boiteau which Mr. Cramer kindly gave me. I fitted to them a curve of the exponential type with an adjustable lower limit to the first eight years of the record, then to six years of the record and then to four years of the record; this record being the same in each case save for the progressive omission of the later observations. The three results were (α being expressed as percentage of new price) $\lambda = 0{\cdot}288$, $\alpha = 8{\cdot}45$; $\lambda = 0{\cdot}391$, $\alpha = 18{\cdot}79$; $\lambda = 0{\cdot}425$, $\alpha = 21{\cdot}45$. Because the data do not quite conform to exponential decay, but show an exceptionally large price drop early on, α gets larger and larger the shorter the record, just as appears for model after model in Table 2 of the paper.

It would be interesting if any figures could be brought forward to show whether this systematic movement of the coefficient α in Table 2 has some such general explanation. Is it the protest of the data at being asked to evaluate coefficients in a law of a type to which they do not really conform? I have mentioned the possibility that the annual proportionate decline in price gets less as the car gets older. This need not of course be a gradual process. It might be simply that the first year's depreciation is normally heavier than that for later years.

Mr. A. STUART: Like the other speakers, I admire the simplicity of Mr. Cramer's model, but I should like to remark on two points arising out of it. To deal with a minor empirical matter first, does he believe the probability of total loss is the same for a car of any age? And is it clear, as he says, that we may regard that probability as constant for all cars? To take an extreme case, is a sports car no more liable to serious accident than a family saloon?

My second point is a theoretical one. Mr. Cramer seems to have used the method of maximum likelihood for estimating his parameters, though he refers to it as a least squares analysis. The method of maximum likelihood in general has theoretical properties superior to those of the method of least squares, but they are not the same method except in the special circumstances of models with independent, homoscedastic, normally distributed errors. The assumptions of normality and homoscedasticity are essential, though Mr. Cramer does not mention them. Without them the two methods are not equivalent, and his analysis would be invalid. Even with the normal assumption, it is worth pointing out that the least squares estimators, though valid, do not have their usual properties of being minimum variance unbiased estimators. For this to be true, the model must be linear in the parameters, and this model is not linear. Furthermore, as Mr. Quenouille pointed out to me, even if the estimators of the parameters were unbiased, it does not follow (and in fact is easily seen to be untrue) that the "prediction" from the model will be unbiased. I think these points need more attention than Mr. Cramer has given them.

Finally, perhaps I may be allowed to mention that in the Research Techniques Unit at the London School of Economics we are about to analyse a sample survey of Motor Vehicle Registration records carried out in July and August, 1957, by the late Dr. Arthur Fox. This was a two-stage stratified random sample of the Vehicle Registrations in Great Britain (excluding N.W. Scotland), and as a result we have, in respect of about 8,500 currently registered vehicles, the Tax Class, the Make and Description, the Weight, Horse Power or Engine Cubic Capacity and the Date of First Registration. Anyone interested in these data will perhaps get in touch with me. Among other things, it will enable us to see

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whether Mr. Cramer's Table 11, which happily relates to the exact time of the survey, is in good agreement with the observations, and to furnish a further test of his model. It will be interesting to see from this whether the estimation biases, to which I have just referred, have any appreciable effect in practice.

Mr. HAJNAL: I should like to raise one small point, which is that Mr. Cramer has not given us very much of the original data. He must have done a great deal of work in collecting these data, and I do hope he will somewhere give us some of the figures. For example, he states that on pre-war data the average duration of life for American cars was 10 or 11 years, compared with 8 or 9 years for British cars. This is contrary to popular opinion on the subject. He may well be right, but it would be interesting to have a summary of such basic data as are available, giving the facts, for example, of the proportion of American cars registered in a given year which were still in operation X years later—a type of figure I gather Mr. Cramer has available.

Perhaps I can extend that plea. It would be interesting to know how far you could get in tackling, by commonsense methods, some of the problems Mr. Cramer has tackled—as, for instance, estimates of the current distribution of cars by age or future scrappage rates. All such things could be obtained by taking rates of wastage for cars of different age and applying them to the basic series of new registrations, exactly as is done with estimates of human population and forecasts of the number of deaths. Those of the necessary rates which are not available from the data could presumably be supplied by commonsense guessing. Where more than guessing seems required, one could see, it such a procedure were set out, just what can be got from the data and what needs further assumptions of a more theoretical kind. I am a little puzzled (perhaps I do not sufficiently appreciate the needs of the economic theorists in this matter) just why Mr. Cramer should have chosen to use an elaborate model, and if he had to use a model why it had to be a stochastic one. The figures he deals with are so large that any chance variation between individual cars, each independently subject to the same probabilities of scrappage, is completely swamped, and it is obvious that the differences between the estimates and the facts, where Mr. Cramer compares them, are not due to chance effects. The repertoire of stochastic models which mathematicians have so far made available for use, is very small. In order to use one, it is often necessary to accept restrictive assumptions which would otherwise appear unreasonable. In the present case the assumptions seem to me outrageous. Without further explanation, one can only vaguely guess what advantage it is hoped to achieve, over commonsense methods, by procedures and computations which are both time-consuming and unrealistic.

Finally, in testing the model against the facts I wonder whether agreements mean anything at all. They do mean a little if you assume Mr. Cramer has the functional forms right; but if you do not assume that, are we not back in the situation in which other mathematical investigators of social phenomena have found themselves? If you have any curve of roughly the right shape with three arbitrary constants, you can usually get a good fit; but there may well be several different families of such curves, corresponding to entirely different conceptions of the underlying processes, which would do equally well.

(Added in writing): Mr. Cramer has kindly supplied me with data on car mortality which he is publishing as an Appendix to his paper. These figures enable me to illustrate my remarks at the meeting with a concrete example, the comparison between the life expectation of cars in the United Kingdom and the United States before the war. Hasty calculations with unfamiliar data are always risky; I can only hope I have not made any serious slips.

From Mr. Cramer's Table A3 anyone used to life tables can calculate in a matter of minutes, without any computing aids, the life expectancies for new cars implied in the United Kingdom surveys for 1936–37 and 1937–38. They turn out to be 8·0 and 8·8 years respectively. (The essential series, the numbers of survivors at successive ages according to the scrappage rates for each age, have been computed by Mr. Cramer. The

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only arbitrary element arises in completing the table at high ages, but this can make very little difference. A life expectancy of $1\frac{1}{2}$ years at 12 years of age has been used; this is in agreement with the run of the survivorship series and with the registration data for pre-1925 cars.)

Computation of an appropriate United States comparison is a little more complex. Since the data start with the 1930 models, scrappage rates for 1937–38 are available only for ages under 8 and this does not cover the bulk of car mortality. Taking the rates for 1940–41 (the last pre-war period) gives a life table ending at 11 years with 443 survivors out of 1,000 new cars. Putting in a life expectancy of 1·66 years at that point (about right from the British data) gives a life expectancy for new cars of 10·0 years. In this life table the mean expected life for cars 8 years old is 3·32 years (compared with 2·44 for U.K. 1937–38). Putting 3·32 years into the American table for 1937–38 gives a life expectancy for new cars of 9·1 years. A similar procedure for 1936–37 gives 10·2 years. In view of the different dates of registration (July against September in each year) the figures for both countries may be regarded as identical for 1937–38.

The computations involved in these comparisons take well under 1 hour on a desk computer. Yet even they carry the discussion well beyond the point to which the elaborate methods of the paper have led, by suggesting two conclusions: (1) While United States car mortality before the war seems to have been somewhat lower in general than that in the United Kingdom, the difference is not nearly so clear cut as Mr. Cramer suggests—the 1937–38 life tables give practically identical life expectancies for both countries. (2) Much of the difference which appears is due to the effect on the United Kingdom data of the high mortality of pre-1930 cars when more than 7 years old. No figures for pre-1930 cars enter into the U.S. estimates (Mr. Cramer's method on the contrary, seems to bring post-1945 scrappage rates into the calculations for the United States).

A proper discussion of these questions would, of course, involve other computations and the consideration of several matters which are neglected in the paper. Examples are: To what extent do changes in registration figures reflect scrappage, (e.g. is it plausible that of the 1934 models in the U.S. $3\frac{1}{2}$ per cent. were scrapped in 1937–38, but none in either 1936–37 or 1938–39)? Were the changes in registration in the counties covered by the U.K. surveys influenced by migration? If the evidence relating to these and similar problems were assembled, we should have a meaningful discussion of the length of car life. All this could be achieved with a fraction of the time which it must have taken to fit an arbitrary set of parameters and then recalculate from them estimates of unknown value for quantities which could have been obtained directly from the data.

Upon a careful discussion of both car mortality and secondhand price data could be based a treatment of the relation between the two and of the underlying factors influencing both of them. Theoretical models may be useful here; but they should surely not be used as a substitute for commonsense discussion of the basic data.

Mr. I. D. Hill: Sir Robert Shone disagrees with Mr. Cramer's assumption that depreciation is basically a question of wearing out of the equipment, and believes that it is due more to obsolescence. It seems to me that this question might be tested by examining the second-hand prices of Volkswagen cars, where the latest model at present in production is still basically of the pre-war design. The question of the existence of a new design reducing the value of the old would therefore not arise.

It may be objected that other makes of car have brought out new models within the period and that this would be a disturbing factor, but it might be possible to allow for this by taking into account what has happened to the price of a new Volkswagen.

Dr. Barna: I wish to raise only one point and this concerns Mr. Cramer's finding that the mortality rates of motor-cars in post-war years were lower than in pre-war years. The research which I am doing in a similar field is far from complete, but it does throw light on this issue. I collected data from industrial firms on the mortality experience of industrial assets and one important conclusion which appears to emerge is that mortality rates in

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recent years were very low. It is generally believed in industry that the rates experienced in recent years were much lower than in pre-war years and it is also thought that present rates may not be a suitable guide to what one might expect in the future.

I feel, therefore, that the suggestion put forward by Professor Stone that the falling off in mortality rates of motor-cars can be explained by a slower rate of technical advance in post-war years does not seem to be valid. In the case of industrial assets generally the converse is held to be true, namely, that technical advance was faster in the post-war years than before.

I feel inclined to agree with Mr. Fisher's suggestion that for an explanation one must look to broad economic reasons—to the state of full employment, increasing demand, long waiting lists for equipment, and so on.

This conclusion may be very disturbing for economists and must be even more disturbing for accountants who have to make estimates for depreciation. It seems to be difficult to define depreciation rates without reference to the general economic climate of the time. It follows from this that depreciation rates are not constant but may go up or down in the course of time as a result of changes in the economic climate or simply as a result of changes in fashions in using new or old equipment.

Mr. T. H. Beech: First, a technical point: it would seem that the choice of numerator and denominator in Table 7 is inappropriate, the difference between pre-war and post-war scrappage being exaggerated by the use of "crude" rates. Such a comparison is affected by the age-structures of the two populations as well as differences in the expectation of life; in our post-war car population we have a high proportion of "children", thus reducing the crude mortality rate quite apart from the difference due to different expectations.

I should now like to take up the wider issue. Despite the above remarks there obviously is a big difference between pre-war and post-war rates of scrappage. It is, I think, a broad economic matter at the root of this. To run a reasonably new car demands capital and income; to run an old car demands only income.

Before the war, when society was more polarized, there was a group of people who had capital and income and replaced cars at fairly regular intervals, and a much larger group, the greater proportion of the population in fact, who had not the income to run a car even if it were given them. In our more equalized society it seems there is a more or less limitless demand for cars by people who have sufficient income to run a car, but are not able to organize themselves to accumulate at regular intervals the capital necessary to buy, even by hire purchase, a relatively new one from time to time. I think that factor is the main cause of the difference in scrappage rates.

Neither has the paper nor have the speakers gone very much into the "sickness" rates of cars, which I feel would repay study in some detail. Even on Mr. Fisher's model, which is most interesting, there is still not taken into account the possibility of replacement of some elements from time to time. The position is not quite similar to the radioactive case where this cannot occur.

Although much has been made of the point that the birth rate of cars is primarily governed by the business demand it is equally true that the private sector determines the time at which a car finally reaches its death and that should be brought out also. Because of the different factors governing the two ends of the life cycle the "expectation of life" may well show considerable instability from year to year and even decade to decade, although gradual changes in the rates of physical decay and of technical obsolescence must influence the long-term trend.

Mr. H. S. Rich: If anyone is going to do some empirical work, as has been suggested, to pursue further the subjects we have heard about, there is one important factor which should get a great deal of attention—the behaviour of dealers. The action of dealers in holding stocks, and their reluctance at times to permit economic forces to lead to the changes they would normally lead to, is important.

Work of this kind would be very useful as a buyers' guide. It is interesting to look

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on this from the point of view of a prospective buyer of a motor-car. You can buy a 1-year-old car which was bought new at £800 for much the same price as a new car at £600 or £650. Most people, when they have that choice, do not do much scientific analysis of the balance which confronts them. How the buyer can be helped by better knowledge is a matter well worth attention by those who are going to carry this subject further.

The following contribution was received in writing after the meeting:

Dr. DURANT: Thomas Huxley on a famous occasion said that a tragedy for Herbert Spencer was having a hypotheses killed by a fact. Cramer makes various reasonable hypotheses and emerges with some tentative conclusions, but it is rather difficult to discover any facts in his paper. This is very largely not his fault because surprisingly enough, whilst the facts about the age structure of the motor-car population are on record, i.e., in the files of the local authorities, no effort has yet been made to collate them for the purpose of achieving such a table. Or put more precisely, many efforts have been made, but they have had no opportunity of being successful.

The only alternative is, of course, to sample the car population. It is not an easy task because the sample must be a random one and it must be sufficiently large to enable a detailed analysis to be made of the results. Only now is this task being undertaken. The Gallup Poll are interviewing 2,000 households each month, chosen on a random basis, and asking, amongst other things, about their ownership of motor-cars. "Owner" must, of course, be interpreted as also including "exclusive use of", as in the case of commercial travellers. This monthly survey means that at the end of a year we will have accumulated somewhere around 6,000 cases. This begins to be big enough to enable a detailed analysis to be made. Since we intend to continue this monthly survey into the foreseeable future we will, before long, also be in a position to build up scrappage rates.

This means that we will be able to apply the test of fact to the conclusions that Cramer has reached on the basis of his hypotheses.

Mr. CRAMER, in reply: I should like to thank all those who have contributed to the discussion. A great deal has been said about forces which are behind changes in the depreciation rate or scrappage figures. I accept much of what has been said, but I did not go into that at all; I have only tried to establish that they are governed by similar forces and move together.

Mr. Fisher has made quite a dent in my argument, and I would rather reply to him in writing.

I have taken the price data from a confidential publication, and I am not sure whether it would be right to reproduce that. But I shall be glad to give the fundamental data in regard to the life distributions.

Mr. Cramer subsequently replied in writing as follows:

Several speakers have noticed that I have not established the validity of the proposed model in the sense of its superiority over alternative models. At the time of writing such models were lacking. Authors such as Kurtz (1930), Boulding (1955) and Boiteux (1956) are not concerned with interpretation; they may well achieve a better description of mortality rates or secondhand prices separately, but they do not deal with the connection between the two which I have been trying to establish. The choice between the heuristic methods employed by these authors and the type of approach I am attempting seems to me mainly a matter of taste. It is clear that I do not share Mr. Hajnal's strong preference for the former. Some of the commonsense methods he advocates have in fact been used by the authors just mentioned; average observed mortality rates, for example, have been used by Boulding (1955). I do not want to criticize these studies, nor Mr. Hajnal's own example, but they appear to have a different object from my own.

Mr. Fisher raises the same point but illustrates the argument by suggesting some ingenious variants of my model which appear to make very good alternatives. By a seemingly trivial variation of the critical quantity level p at which cars are assumed to be scrapped he

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arrives at a wide class of models, each of which would be compatible with the general characteristics of the observed secondhand prices and mortality rates. But the estimates of λ and ν from the latter will vary considerably according to the value of p selected. This is a welcome extension of my model. Unfortunately, the present data are of insufficient accuracy, and the period they cover is too short, to discriminate between Mr. Fisher's variants and the original model. But in this respect we may perhaps derive some comfort from the promises of better information on car mortality held out by Mr. Stuart and Dr. Durant.

Clearly, however, Mr. Fisher himself regards his suggestion not so much as a helpful extension of the assumptions but rather as a demonstration of the arbitrariness of the estimates of λ which I have derived from the mortality data. It is true that these estimates depend on the "scrap level" selected. But the changes in assumptions which produce the variations in λ in Mr. Fisher's example are not as trivial as they seem. A critical level of one or two units may seem very much the same thing as a level of zero, but it is not so in fact. The units are arbitrary and can only be compared if we relate them to the quantity of a new car which is represented by their initial number ν. As the estimate of ν varies with the level of p assumed, the resulting changes are not negligible. The representation in discrete units is, as I have stressed, a mere analytical device, adopted for the sake of convenience; if we assume a critical level p, this means that the critical level in a continuous representation would lie between $\frac{p}{\nu}$ and $\frac{p+1}{\nu}$.

For the three cases treated by Mr. Fisher these limits work out as follows:

p	*Critical Level as a Fraction of New Car Quantity*	*Estimate of* λ
0 . .	0–·014	·53
1 . .	·035–·070	·32
2 . .	·086–·129	·24

Mr. Fisher's second suggestion is that the price data do not conform to the model but depart systematically from it in the specific manner which he describes for the Renault data. I have gone through the pre-war British data to find out whether this is a general characteristic of the price series. To do so I have used the same price data as in Table 4, and I have taken the values of α as given. One can then easily compare the value of λ for various age periods. It appears that the proportionate price drop in the very first year is often (but not always) somewhat larger than in later years, in particular for the cheaper cars. There is, however, no evidence that the rate of λ varies systematically with age after this initial drop has occurred. For 20 cars of the 1932 and 1933 model—10 of each—the decline in price can be compared for the third and sixth years; 4 of each model year show a diminishing decline and the remaining 6 show the opposite.

The initial extra decline in price during the very first year may partly be due to the difficulty of determining the age of the cars offered on the secondhand market in the fourth quarter after the middle of the "model year" to which they belong. It may also be the result of the transition from the first hand to the secondhand market. This first-year price drop does not affect the estimates of λ from "cross-section" data (Tables 4, 5 and 8), as they are based only on price differences at later ages.

A second aspect which has caused some uneasiness is that I have not given an interpretation of the model in terms of the underlying factors which determine the rate of depreciation and would account for the changes which have occurred in it. I realize from the remarks of several speakers—Sir Robert Shone, Mr. Fisher, Mr. Hajnal—that I have perhaps made it insufficiently clear that the presentation of the model in physical terms such as "quantity" and "component" is an analogue, meant to derive both price and life distributions from a single set of assumptions. This is achieved by the introduction of a stochastic process; it has to be stochastic in order to account for the variation in the duration of a car's life, and not (as Mr. Hajnal appears to think) to account for the discrepancy between the observed values and those specified by the model.

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The interpretation of the depreciation rate in terms of obsolescence or wear and the choice of further determinants of its value are outside the scope of the present paper. Clearly these questions present interesting economic problems. Nearly all speakers have touched on the subject, and I am grateful for their interesting (though sometimes conflicting) suggestions. More research will, however, be needed before a definite opinion on these matters can be given.

Mr. Stuart has questioned the method of estimation I adopted. I deliberately refrained from making explicit assumptions about the statistical properties of the errors and disturbances concerned. To do so seemed unnecessarily pretentious as, in the present case, such assumptions are inevitably rather arbitrary and in fact largely dictated by convenience. It is true, however, that both the computational methods and the use of variances and confidence regions imply the assumptions of normality and independence which Mr. Stuart quotes.

Finally some minor points. Mr. Beech thinks that after the war the car population contained more new cars than previously. In view of the large number of pre-war cars I think the converse held, at least until recent years. Mr. Stuart questions my assumptions about μ. I made the simplest assumptions which worked (in a way) but I would accept a variation of μ with age or type of car. It does not, however, enter into the calculations for individual makes, which are based on prices.

As a result of the ballot taken during the meeting the candidates named below were elected Fellows of the Society:

Ronald William Elliott
George Robert Fraser
Margaret Elizabeth Gibbons
Ram Krishna Gupta
Colin Sidney Harman
Thomas Henry Hill
Khin Maung Lwin
Knut Hakan Medin
K. M. Nyunt
Geoffrey Ramsden
Paul Edward Stonham
Patricia Ann Tapp
Elizabeth Anne Markham Tate
David Theodore Welch
Herbert Timothy Young

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The British Propensity to Save

By L. R. Klein

It would seem fitting to enquire into the realism and usefulness of Lord Keynes's abstract and subjective concept "the propensity to save" in his own country. A great amount of effort has been devoted to measurement of America's and other countries' savings functions, but comparatively little has been possible in Britain because of unavailability of data. In this paper, I shall report on researches into the data collected in the Oxford Savings Surveys with a view towards establishing whether or not a stable relationship exists that can properly be called the British savings function and to make objective measurements of the propensity to save implied by such a function.

This paper draws on the work of other investigators who have previously dealt with the empirical estimation of the savings (or consumption) function for the United Kingdom. Noteworthy in this respect are the studies of aggregate time series data by Clark (1938), Radice (1939), and Stone and Rowe (1956). A different approach in connection with the data in the Oxford Savings Survey is taken by Fisher (1956).

The Crude Treatment of Time Series Data

Stone has used annual time series of consumption, income, and other variables covering both pre-war and post-war years to estimate aggregate equations for savings and durable consumption. His main findings, apart from the actual numerical values estimated, are that the factorial distribution of income and lags in consumer adjustment are important characteristics in the propensity to save in the U.K. Another point of general interest in his findings is the structural homogeneity between the pre-war and post-war periods. A function fitted to pre-war data alone extrapolates well into the regions containing the post-war observations. To many investigators this finding is wholly unexpected.

The microeconomic analysis of the survey data presented in this paper will, it is hoped, throw some light on these aggregate findings. As a preliminary step, however, it may be useful to take a cruder look at the aggregative data. The long-run gross relation between consumption and income can be visualized readily from some data recently made available on national income and expenditure since 1870 (Jeffreys and Walters, 1955). The data graphed in Fig. 1 are estimates of the flow of goods and services to consumers, *per capita* (consumption *per capita*), and net national income, *per capita*. They are averages of overlapping decades. Thus the data are smoothed, and this is why the relationship plotted is termed the long-run relationship. Short-run lags of adjustment stressed by Stone are not particularly relevant with these data. The distribution of income among factor shares would be worth considering, but relevant data are not available, and we have plotted the gross relationship on a single two-dimensional basis. Another important point to be noted about this graph is the absence of correction for tax and transfer payments. We do not use a concept of disposable income.

The points corresponding to decades including a large number of war years, in either World War I or II, are circled and seen to lie substantially below a normal pattern of

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relationship, although the decade 1915–24 is something of an exception and does not lie far below the general scatter of points. The same is true of the eight years immediately following World War II. These years will be discussed in more detail below.

Excluding the circled observations, we have fitted a simple linear function to these decade data by the method of sub-group averages. From the first six pairs of "normal" observations and from the second six we have computed mean consumption and mean income. These averages define two points, through which our fitted line passes. Under

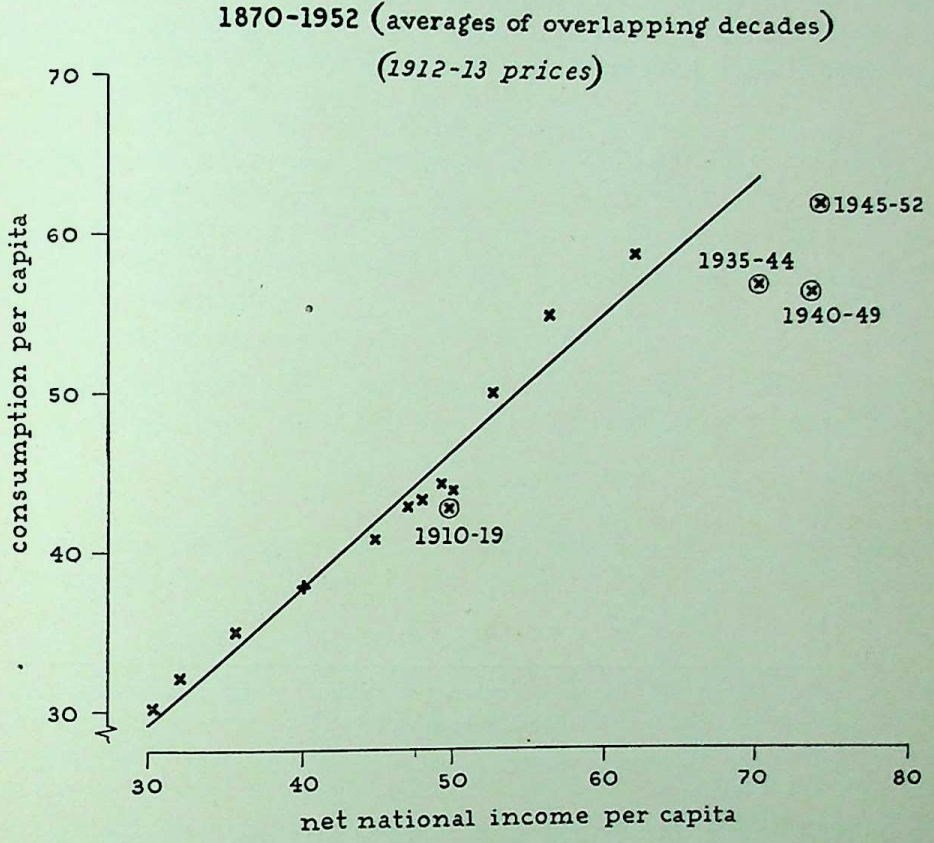

FIG. 1.—Long run consumption-income relationship.

certain mathematical conditions, this method provides an unbiased (in the technical statistical sense) estimate of the underlying relationship, calculated as

$$\hat{C} = 3{\cdot}02 + 0{\cdot}87Y,$$

C = Consumption *per capita* in 1912–13 prices,

Y = Net national income *per capita* in 1912–13 prices. . . (1)

The marginal propensity to save is 0·13 ,and the long run function has a constant term (3·02) although the sampling error is so large that it does not differ significantly from zero.

In view of Stone's findings, from a more careful analysis of time series data, that World War II and its aftermath did not rupture the parametric structure of the propensity to

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save, we may consider the coefficient of 0·13 to be roughly indicative of the long run propensity to save. This coefficient is smaller than that obtained from the annual post-war series, in which personal income is corrected for tax and transfer payments, but agrees more closely with the cross-section estimates from the post-war sample surveys. The survey data also use the disposable income concept. The main tax changes are those occurring in the years since 1939, the last year used in the computation of the line fitted in Fig. 1.

Since the end of World War II, savings as estimated in the national income accounts (personal income minus expenditure) have shown an interesting pattern. Until 1952 the total fluctuated at a low level and then grew rapidly from £219 million in 1951 to £724

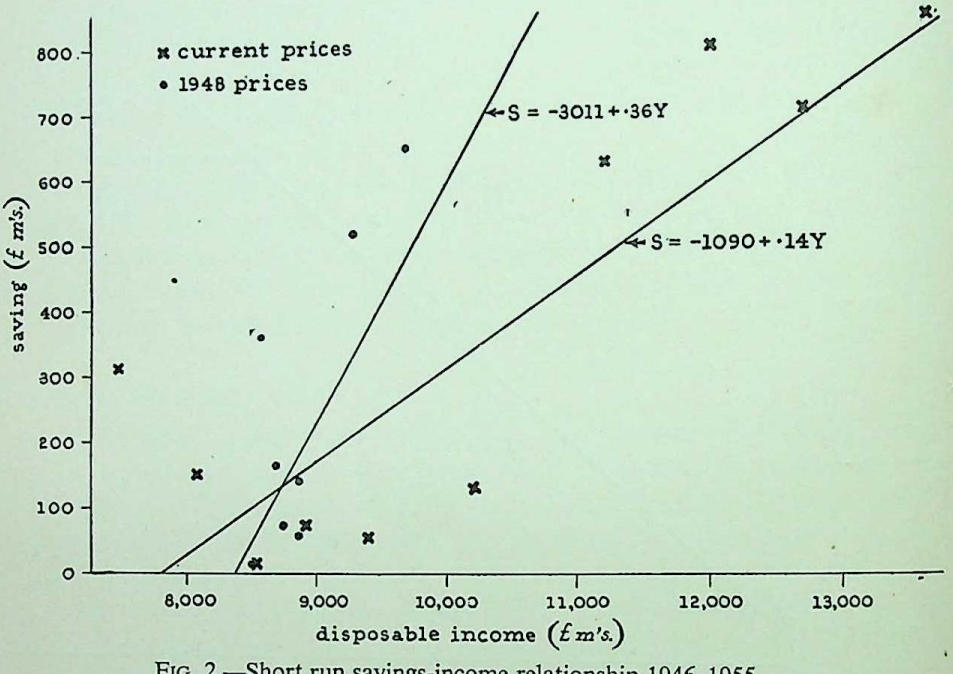

FIG. 2.—Short run savings-income relationship 1946–1955.

million in 1952. Since then savings have remained high and increased slightly. The low savings of the early post-war years are undoubtedly explained in part by the task of reconstruction and reorganization of the economy, the spread of inflation, and the introduction of vast social services.

Between the period preceding 1952 and the succeeding period, there is a large jump in personal savings accompanied by a steady growth in income and prices. The short run relationship depicted as a scatter diagram of annual savings-income points for the ten years 1946–55 would seem to have an extraordinarily high gradient due to the raising of the general level of savings after 1952. This is indeed the case with deflated data; i.e., savings (S) and income (Y) divided by the price index of personal consumption 1948 : 100. A line, fitted, as before, to sub-group averages is

$$S = -3011 + 0{\cdot}36Y \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In this case S and Y are aggregates measured in millions of pounds. If we say roughly that prices in 1948 were three times as high as in 1912–13 and that the population of the

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U.K. is about 50 million persons, we can transform (2) to a basis comparable with (1). The variables are to be interpreted as amounts *per capita* in prices of 1912–13. The slope parameter remains unchanged and the constant term is about — 20. This is written as a savings function; hence the constant term is negative. This post-war estimate of the propensity to save has, therefore, a lower position and steeper slope than that implied by (1).

In computing (2), the ordinary estimates of personal savings and personal income from the national income accounts were adjusted by subtraction of statutory depreciation allowances for professional persons, persons other than sole traders and partnerships. In addition, direct taxes and national insurance contributions were subtracted from personal income to get disposable income.

The slope would not be so steep and the position of the function so low if the analysis were confined to the more normal period since 1952; however there are too few observations to say anything statistical about the behaviour pattern in this period. It is worth noting that we come much closer to the earlier results in (1) if the values of S and Y are not deflated by the index of consumer goods prices. Fitting the undeflated data by subgroup averages, we estimate

$$S = -1{,}090 + 0{\cdot}14Y. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Using the same adjustment as before for population and changes in the price level since 1912–13, we estimate that the constant term should be transformed to about

$$-\frac{1{,}090}{150} = -7{\cdot}3$$

to be made comparable with that in (1). Current price data of recent years are thus not far off the long run savings pattern.

The Nature of Sample Survey Data on Savings

The time series aggregates are studied crudely in the preceding section merely to establish a frame of reference in which to judge the more detailed cross-section estimates of the propensity to save.

First it will be useful to consider briefly the nature of the data used in both types of studies. The data on savings in the national income accounts are calculated as the residual difference between personal income and expenditure estimates. For as many components as possible (changes in individual assets and liabilities) Saunders (1955) has constructed direct estimates of savings, but is not able to collect enough information to make comprehensive estimates of savings as Friend (1954) and Goldsmith (1955) have done in the case of personal savings in the U.S.A. Data from the Oxford Savings Surveys, like those in the American Surveys of Consumer Finances, attempt to estimate personal savings in nation-wide samples of consumer units by the direct method (Lydall, 1955), (Klein, 1955). These data differ from the national income data in significant respects which must be fully understood if they are to be used to establish statistical properties of the propensity to save that can be made comparable with results derived from the time series aggregates.

The most striking differences between the Oxford Savings Survey data and the National Income data on savings are in their numerical size. In the most recent survey* (early 1954),

* The results of the 1955 survey are generally similar, but this was not an Oxford survey; it was carried out by the Central Statistical Office through the facilities of the Social Survey.

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savings were £11 per income unit. There are about 24 million such units in the U.K.; therefore this average corresponds to a national total of about £264 millions. During the calendar years 1953 and 1954, the Central Statistical Office estimates savings to have been £850 million and £729 million respectively. Oddly enough, the two sets of figures are not in basic disagreement after one goes through a fairly exhaustive reconciliation.

The Savings Survey deals with "people"—actually those living in Great Britain and outside institutions. The "personal" sector in the national income accounts covers all people in the U.K., whether or not in institutions, and in addition many "collective persons" such as non-profit-making bodies, trust funds, etc.

Even if the two sources of savings data could be brought into agreement on the universe represented there would remain differences in the concept of savings. Respondents in the Savings Surveys are not asked about currency holdings. It is impracticable to treat the interest on Savings Certificates as either income or savings in the surveys. In connection with life assurance payments, each person in a survey is not credited with savings for an amount equal to his share in the accretion of insurance company reserves. Instead, full premium payments are assumed to represent savings. In all these respects, the national income estimates are able to sort out the contributions to savings under a widely accepted set of definitions. *Employer* contributions to pension funds are part of aggregative savings estimates, but not part of the survey estimates. This is one of the major discrepancies between the two sets of data. Other technical differences exist in the two types of savings estimates. These are concerned with the treatment of annuities, capital transfers, expenses of property transfer, and depreciation on farms, professional practices, other sole proprietorships, and partnerships.

The actual numerical reconciliation following through all the differences in universe and concept are too detailed to report in full. Having made the reconciliations by redefining aggregate personal expenditure and personal income to bases implied in the survey data,* we find that the national income residual estimate of savings at about £800 million is reduced to about £300 million when placed on the survey definition. If recent annual changes in notes and coin outstanding could be allocated to persons the figure would be slightly lower.†

We might reasonably accept, as a working hypothesis, that the two basic sets of savings data are mutually consistent. They are, however, conceptually different, and this must be kept firmly in mind for purposes of comparison.

Both the national income accounts and the savings surveys are consistent in that they exclude capital gains or losses from the concepts of savings and income. Possibly behaviour of individuals is significantly affected by such gains and losses, whether they are realized or not. For statistics of the economy as a whole, there is good reason to exclude capital gains and losses, but these reasons lose force when applied to individuals or specific sub-groups in the population. It is felt that in recent years, some people have maintained their former standards of living in spite of inflation by spending capital gains. In this way they have maintained the nominal capital value of their holdings and yet not reduced living standards. By the conventions of social accounting, they have spent more than their

* Since expenditure data are not collected in the Oxford Savings Surveys, the expenditure concept is merely implied by the savings and income concepts.

† The amount of notes and coin outstanding (outside banks) increased by £78 million in 1953 and by £94 million in 1954.

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income and apparently dis-saved, but they have not necessarily drawn down the value of their net assets.

Another respect in which the savings surveys have followed national income accounting practices and thereby provided perhaps a distorted view of individual savings behaviour is in the treatment of private companies. Many of these are wholly owned by one person or a limited family group. All the profits are not withdrawn for personal use, but are allowed to accumulate as company savings. In savings statistics these are classified as business savings in the corporate sector and not as personal savings; whereas many people undoubtedly regard them as essentially a personal form of savings. This savings device is used for tax advantages as is the spending of capital gains.

In the savings surveys, *capital transfers* consisting of gifts, inheritances, gambling winnings, life assurance benefits, etc., are debited to the savings account of the recipient and credited to the savings account of the donor. In a random sample of the population as a whole, we should realize mutual cancellation of debits and credits. We obviously cannot conduct personal interviews with donors of inheritances and some institutions such as life assurance funds. Some justification for the accounting procedures used can be made if we look solely at sample averages or relationships within the whole sample. Individual figures and those of selected sub-groups can however be seriously distorted by these procedures.

A Model of Savings

Two current views about people's savings behaviour which we rejected for research strategy in this study are: (1) Savings are a residual component of consumer budgets. (2) The savings concept that provides the most stable pattern in relation to associated variables should be more aggregative than that customarily used and should be the "dependent" variable in the savings function.

In this paper, our model of savings behaviour will be based on a view of "active" savings decisions. The decisions to purchase an assurance policy, to build a residence, to buy shares, and similar actions are as carefully planned and thought out as decisions to purchase food, clothing, fuel, entertainment, and all the other items in a consumer's budget. Some conponents of savings will, for some people, be residual elements. It is plausible to assume that some people simply let their bank balance fluctuate by the amount of spread between their income and active purchase decisions.

A distinction of the first importance in the work that follows will be made between *contractual* and *discretionary* (active) savings. We have just pointed out that provision for life assurance is an active savings choice, but this is true only for the initial decision to enter into an arrangement over a span of several years. The annual premium payments following this initial decision are recurring contractual obligations and no longer involve new decisions. In the Oxford Savings Surveys, payments on life assurance, superannuation, and mortgage debt are all classified as contractual savings. Other obligations of a contractual nature would be repayment of hire purchase debts, but the classification, as it now applies, covers only long term contracts.

Contractual savings have special characteristics. They can be only positive. They bear a fairly stable relationship to income. Having made the research decision to treat them separately from other types of saving, we find ourselves going in the direction of decomposing rather than aggregating types of savings. If savings are defined as the rate

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of change of wealth, all consumer expenditures that add to wealth belong in an aggregative measure of savings. This is especially important for items of *durable consumption*, and many investigators have noted that savings, conventionally defined, plus durable consumption are more highly correlated with income and other explanatory variables than are savings without the addition of durable consumption. The trouble with the more extensive savings concept even if it could be argued that it leads to more stable relationships,* is that it is unrevealing. Often, policy and theoretical considerations lead us to enquire about the behaviour of types of savings whose pattern is covered up in the more aggregative concept. It is immediately obvious that the problems of business cycle control cannot be properly treated without distinguishing between the more conventional definition of savings and expenditures on consumer durables.

We shall, in our model, deal with two main types of savings, discretionary and contractual. They, together, add to total savings as defined in the Oxford Savings Surveys.

$$S = S_D + S_c \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

The components of total savings for an individual unit may be summarized as follows:

Contractual Savings S_c	*Discretionary Savings* S_D
Life assurance payments	Change in national savings
Mortgage repayment	Change in other liquid assets
Employee superannuation contribution	Change in indebtedness
	Purchases less sales of securities
	Purchases less sales of house or other real property
	Retained business earnings (private and public companies excluded)
	Change in outstanding loans
	Net transfers of cash capital to others

Discretionary savings, S_D, are assumed to be related in a multivariate equation to disposable income Y, last year's disposable income Y_{-1}, liquid assets held one year ago L, windfall receipts W, size of income unit N, age of income unit head A, and contractual savings S_c. The actual form of the relationship to be estimated is

$$S_D/Y = \alpha_0 + \alpha_1 \log Y/N + \alpha_2(L/Y)(Y_{-1}/Y) + \alpha_3 W/Y + \alpha_4 A + \alpha_5 S_c/Y + u. \quad . \quad (5)$$

Some words of explanation are obviously needed to justify the choice of this particular equation.

We define the basic variable studied as the savings-income ratio rather than the absolute amount of savings. This is in order to remove, by means of a fairly simple device, heteroscedasticity from the data. Heteroscedasticity, or lack of uniformity of variance, arises from the well established point in consumer surveys that variability of total savings (or spending) grows with the size of income. The percentage of income saved tends to show uniform variability in all income classes except possibly at the lowest levels. A linear combination of income and assets would probably do a better job of removing heteroscedasticity. It would especially take care of cases with low income and substantial wealth. The percentage of income saved is, however, a variable with intuitive significance and removes nearly all heteroscedasticity; hence it is used in the present calculations.

* The higher degree of correlation obtained with the more aggregative concept is a matter of fact, but we need not identify degree of correlation with degree of stability.

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The percentage of income saved depends linearly on the logarithm of *per capita* income in (5). This gives good empirical graduation to the non-linear savings-income relationship with the marginal propensity to save an increasing function of income level. By expressing income in *per capita* terms, we take direct account of family size.

Liquid assets (as a percentage of income) and income change are combined into a product variable

$$(L/Y)(Y_{-1}/Y)$$

Individually these two variables are by now familiar ingredients in the propensity to save. The product form connecting L/Y and Y_{-1}/Y brings out an *interaction* effect on savings. We might argue that a fall in income ($Y_{-1}/Y > 1$) accompanied by holdings of liquid assets is more likely to lead to a maintenance of former consuming standards and lower savings than if unaccompanied by such holdings. Similarly, an income increase ($Y_{-1}/Y < 1$) is less likely to lead to higher savings if liquid assets are already large than if they are small or non-existent. The particular way of expressing this combined variable

$$(L/Y)(Y_{-1}/Y)$$

gives a one-dimensional scaling along the positive axis in agreement with *a priori* expectations about the joint effects of liquid asset holding and income change.

The next term in equation (5), $\alpha_3 W/Y$, brings in a variable called "windfall" receipts. These are capital transfers in lump sum consisting of gifts, inheritances, insurance benefits, gambling winnings, and similar payments. Steadily recurrent gifts and annuities are not included.

Some people tend to save their windfalls, and others spend them on balance, but there is more of the latter than the former type of behaviour; therefore we expect to find a negative relation between S/Y and W/Y.

Age of income unit head, A, is the next variable in (5). It is frequently cited as an important factor in individual savings behaviour. The effect of age is usually thought to be non-linear, with savings rising throughout the lower age groups to a maximum point in the middle age range and then falling in the oldest group where people often live on capital to some extent. The youngest people are just beginning to build up their estates and can save only limited amounts for that reason. The older people are drawing down the capital in their estates. Thus we find a "humped" age pattern of savings. There are occupational differences in the savings–age relation, but the "humped" pattern is typical of the bulk of the population. Income follows a similar pattern through an individual's lifetime; consequently the savings–income ratio is not as severely non-linear in its relation to age as is the total value of savings.

The final variable in (5) is contractual savings and is introduced in an attempt to tie together a consistent picture of the various steps in an individual's saving process. After the initial decision has been made to enter into a contractual arrangement such as mortgage, life assurance, or superannuation contract, future payments become obligatory and a first claim upon resources.* It is possible for one to default on a contract and fail to make payments. It is even more possible for one to pay more than the contracted amount at any time; nevertheless the basic tendency is for one to continue paying at the agreed upon rate.

* In the case of superannuation, the scheme is frequently tied to a job, and the individual exercises little choice in the short run, even at the initial stage.

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If the coefficient of S_c is -1, then we have £ for £ substitution between contractual and non-contractual savings. It would be just as well to use S as S_D (with S_c as an explanatory factor) as dependent variable in equation (5). If the coefficient, on the other hand, is such that

$$\frac{\partial S_D}{\partial Y}\Big/_{S_c=\text{const.}} = -\frac{\partial S_D}{\partial S_c}$$

one would do just as well to subtract contractual savings from income to get a closer approximation to the current flow of net spendable funds. On the basis of our calculations, we shall conclude that the two types of savings are substitutable for each other, though not at a rate of £ for £, and that the marginal coefficient of contractual savings is not the negative of the marginal income coefficient.

At the end of equation (5) we have added an error term, u. Underlying our statistical methods is the assumption that this is a random variable consisting in our sample of mutually independent values drawn from a distribution with constant variance.

A slightly extended form of equation (5) may be considered in which all the terms are preserved as already written, but an additional term $\alpha_6 D/Y$ (D = durable consumption) is added. As in the case of S_c/Y, we could interpret a value of -1 for α_6 as meaning that durable goods are substitutable £ for £ with savings. In this case, the broader definition of savings, extended to include expenditure on durables, would be appropriate.

Equation (5) shows how contractual savings affect discretionary savings. Another step in the causal chain is the "explanation" of contractual savings themselves. A linear, proportional relationship between contractual savings and income is a good first approximation, since contractual savings are non-negative and tend to vanish as income approaches the zero level. They differ markedly from discretionary savings in this respect. To put the matter another way, we observe a significant positive correlation between contractual savings and income, but only an insignificant correlation between the percentage of income saved contractually and income. We immediately extract the income effect on contractual savings by using the ratio S_c/Y as a dependent variable and, at the same time, correct for the presence of heteroscedasticity. The assumed equation is

$$S_c/Y = \beta_0 + \beta_1 A + \beta_2 \log N + \beta_3 M + v. \quad . \quad . \quad . \quad (6)$$

Besides the variables already introduced, we have M = mortgage status (1 = has mortgage; 0 = does not have mortgage) as an explanatory variable.

The role of age in contractual savings is much like that in discretionary savings. Young people are just beginning to acquire life assurance property. They may contribute modest amounts to a superannuation scheme, but they generally live in rented dwellings. Older people are often living on superannuation benefits and no longer contributing. They frequently have paid up the full amount of their life assurance and have retired their mortgage debt. The main contractual savings are in the middle age groups. The variable S_c/Y has a humped pattern over the age range, but to a linear approximation, as in the case of S_D/Y, declines with age.

At first glance it would appear that the contribution of log N is similar in equations (5) and (6), but reflection on the savings processes involved reveals the important difference that the effect of log N on S_D/Y is negative ($-\alpha_1$) and on S_c/Y is positive (β_2). This is an important reason for decomposing savings into components. The burden of additional

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family members makes saving generally more difficult, thus leading to a negative coefficient in (5). On the other hand, the addition of more family responsibilities makes life assurance coverage for the head of the family more urgent and mortgage payments more likely. The effect of growing family size on contractual savings should begin to lose force after the two most important qualitative changes, marriage ($N = 2$) and children ($N = 3$ or more). The logarithmic transformation gives a reasonable degree of non-linearity to the effect of family size.

Mortgage status, for the population as a whole, is a slowly changing and predictable variable. In a cross-section sample it is one of the major factors accounting for contractual savings. It is, in fact, almost a part of contractual savings by itself, but we have not used amount of mortgage as a variable—simply a coded magnitude indicating whether the subject has or has not a mortgage.

The final equation in our small model of individual savings and expenditure is an equation for durable consumption

$$D/Y = \gamma_0 + \gamma_1 A + \gamma_2 L/Y + \gamma_3 H + w. \quad . \quad . \quad . \quad (7)$$

The new variable in this case is H = home ownership ($H = 1$ if home-owner; $H = 0$ if otherwise). For some calculations we shall include in D only major household purchases consisting of washing machines, refrigerators, television sets, motor cars, motor cycles, and similar large objects costing £25 or more.* In other calculations we have included home decorations and repairs also as a part of D.

Durable consumption is like contractual savings in that it tends to bear a linear proportional relation to income; therefore we eliminate the income effect immediately by transforming to the ratio variable D/Y.

The effect of age on durable consumption is generally similar to that in the equations for both discretionary and contractual savings. In the case of durable consumption, however, we have to give an important place to liquid asset holdings. These funds are available for major purchases, such as occur in D, and especially for down payment in case credit is used. One might think that capital transfers, W, are equally important but we have found no significant results with the use of this variable in (7).

In equation (5), the marginal asset effect is negative, but in (7) it is positive. This again is reason calling for the disaggregation of savings, or the distinction between savings and durable consumption. The interaction between liquid asset holdings and income change used in (5) does not seem to be called for in (7). One could, however, make a separate case for the independent significance of income change in durable consumption. In some calculations an income change variable has been added.

Many durable goods are closely related to the home, and are more likely to be bought by individual consumers who are home owners than by those renting. This is even more strongly so in the case of decorations and repairs; hence we introduce the dichotomous variable H to distinguish between home-owners and other income units.

Estimates—Grouped Data

One of the simplest approximations to a relationship purporting to be the propensity to save would be the ordinary Engel curve of total savings, including both contractual

* In the 1954 Survey, items costing £10 or more are included.

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and discretionary forms, from a sample survey. Three such Engel curves from the Oxford Savings Surveys of 1952–54 are shown in Fig. 3. Apart from the year-to-year stability of the savings-income relationship, these Engel curves of savings individually give the appearance of tightness of relationship. The average points (averages of savings and income by income classes) lie close to a smooth curve, and there seems to be little variation to be accounted for by variables other than income.

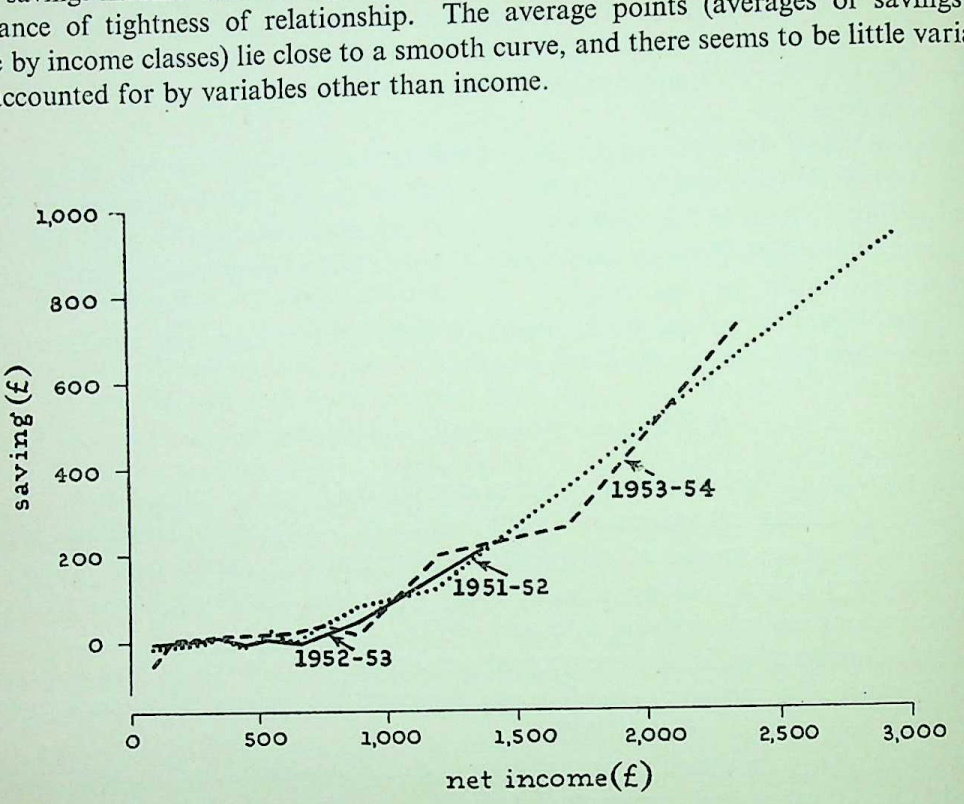

FIG. 3.—The savings-income relationship from three surveys.

Relationships among averages have the advantage of showing clear patterns of relationship by taking random variations out of the data. They are, however, deceptive in concealing a large amount of underlying variability. From a statistical point of view, the use of sub-group averages cuts down seriously on the number of degrees of freedom. On the basis of our hypothesis that individual savings behaviour is a highly multivariate process, we shall base our final conclusions on results obtained from totally ungrouped data (each income unit treated as a separate observation unit) in which there are adequate degrees of freedom to analyse large numbers of variables. It is very useful to approach these highly varied individual observations through the smoothed patterns in the grouped data. The estimates of the propensity to save from grouped data are unbiased, though not statistically efficient, and are excellent guides to the basic relationship provided one does not misinterpret the excessively high correlations they produce (Prais and Aitchison, 1954).

In grouping data for computing means of variables, one could group by an n-fold cross-classification of n explanatory variables. This would have involved a large number of cells and extra work. We, instead, grouped all variables to be studied by one main

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classification—income.* The basic income groups used in the Oxford Savings Surveys are (in £'s) 0–99, 100–199, 200–299, 300–399, 400–499, 500–599, 600–699, 700–799, 800–999, 1,000–1,499, 1,499–1,999, 2,000 and over.

The 1953 Survey tabulations are more complete; therefore all the variables used were averaged for the 12 gross income groups in this sample. The 1954 tabulations were less fully completed at the time these computations were made and some variables were not averaged over the relevant income groups. The main results of the estimated model from the grouped data are

1953–54 Surveys (two low income groups excluded)—

(5.e.1)

$$S_D/Y = \underset{(0\cdot13)}{-\,0\cdot61} + \underset{(0\cdot07)}{0\cdot27}\log Y/N - \underset{(0\cdot039)}{0\cdot048}\,L/Y + \underset{(0\cdot00195)}{0\cdot00043}A - \underset{(0\cdot50)}{0\cdot68}\,S_c/Y$$

$$\frac{\partial S_D}{\partial Y} = 0\cdot09.$$

(5′.e.1)

$$S_D/Y = \underset{(0\cdot18)}{-\,0\cdot60} + \underset{(0\cdot07)}{0\cdot29}\log Y/N - \underset{(0\cdot038)}{0\cdot040}\,L/Y - \underset{(0\cdot00196)}{0\cdot00047}A - \underset{(0\cdot53)}{0\cdot33}\,S_c/Y - \underset{(0\cdot30)}{0\cdot46}\,D/Y$$

$$\frac{\partial S_D}{\partial Y} = 0\cdot13$$

1953 Survey—

(6.e.1)

$$S_c/Y = \underset{(0\cdot025)}{-\,0\cdot019} + \underset{(0\cdot00043)}{0\cdot00074}\,A + \underset{(0\cdot021)}{0\cdot024}\log N + \underset{(0\cdot03)}{0\cdot14}\,M$$

$$\frac{\partial S_c}{\partial Y} = 0\cdot04$$

(7.e.1)

$$D/Y = \underset{(0\cdot08)}{0\cdot24} - \underset{(0\cdot0021)}{0\cdot0053}\,A + \underset{(0\cdot039)}{0\cdot065}\,L/Y + \underset{(0\cdot045)}{0\cdot087}\,H$$

$$\frac{\partial D}{\partial Y} = 0\cdot01$$

* The readily available tabulations were by gross income classes in the 1953 Survey and net income classes in the 1954 Survey.

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These equations are all estimated as least-squares regressions. They are weighted regressions since each set of means for an income class is weighted by the (weighted) relative frequency of income units in that class. The sampling errors indicated in parentheses beneath the associated coefficients are indicative purely of the relative significance of the different variables, since there are few degrees of freedom in this part of the analysis.

The figures in equation (5.e.1) look fairly reasonable as estimates of (5). With these grouped data, accurate measures of income change were not available; therefore we used simply L/Y instead of the interaction term $(L/Y)(Y_{-1}/Y)$ inserted in (5). The coefficient of age, A, is positive in (5.e.1) but negative in (5′.e.1). Both coefficients are small and subject to a large margin of error. Income is obviously the most important of all the variables in the first two estimated equations. The coefficient of liquid assets is reasonable, but this variable does not seem to carry the same weight as income in savings decisions. The coefficients of S_c/Y and D/Y, though not established with the greatest firmness, suggest that there is not absolute £ for £ substitution between S_D and S_c or D, on the average. Data on windfalls were not used in these exploratory calculations. The marginal propensity to save,

$$\frac{\partial(S_D + S_c)}{\partial Y},$$

appears to agree with some of the aggregative time series values presented earlier. The sum of the two estimates in (5.e.1) and (6.e.1) gives a figure of 0·13, to be compared with a point value of 0·14 computed from undeflated post-war annual data.*

In the first two estimated equations (5.e.1) and (5′.e.1), the Surveys of 1953 and 1954 are pooled into 24 income groups. Discretionary savings are, however, so strongly negative in the bottom income class of each survey, that we omitted each of these two extremes and used only 22 sets of means in the least-squares regressions. The next two equations (6.e.1) and (7.e.1) were estimated from the 12 income groups of the 1953 Survey alone since some of the means were not available at the time from the 1954 Survey.

In the case of contractual saving the age coefficient is a small magnitude of the wrong sign and most of the correlation depends on the mortgage variable M alone. The coefficient of age as well as of the other variables in the durable goods equation (7.e.1) appears to be reasonable. The marginal propensity to spend on durables is not large, and this value is not out of line with the increment in

$$\frac{\partial S_D}{\partial Y}$$

as D/Y is added as a variable in the equation for discretionary savings.

The small number of class means imposes a severe limitation on analysis of this type. We can go a step further, however, without reaching the extreme of no grouping. We arranged the data from the 1953 Survey into 55 sub-groups of income per person (Y/N) and liquid asset-income ratio (L/Y). In this grouping there were 3 extreme classes, which were omitted in the regression calculations. The computations with this arrangement of the sample values provide many more degrees of freedom and also give a rough illustration of the effect of different ways of grouping. In this set of group means, data on income change and windfalls were included.

* In (5.e.1), the marginal propensity to save is computed so as to take into account the relation between S_c and Y from (6.e.1). When using (5′.e.1), the relation between D and Y from (7.e.1) must be accounted for as well.

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(5.e.2)

$$S_D/Y = \underset{(0\cdot12)}{-0\cdot45} + \underset{(0\cdot05)}{0\cdot19}\log Y/N - \underset{(0\cdot010)}{0\cdot032}\,L/Y + \underset{(0\cdot0008)}{0\cdot0010}\,A + \underset{(0\cdot190)}{0\cdot033}\,\frac{Y - Y_{-1}}{Y} - \underset{(0\cdot17)}{0\cdot31}\,W/Y - \underset{(0\cdot60)}{0\cdot80}\,S_c/Y$$

$$\frac{\partial S_D}{\partial Y} = 0\cdot10 - 0\cdot80\frac{\partial S_c}{\partial Y}$$

The closest comparison is with (5.e.1), although the present equation includes two extra variables not treated in the previous estimates. The point estimate of the marginal propensity to save is somewhat lower and small differences are readily discernible in the several coefficients, but none of these is likely to be significant. In equation (5.e.2) we find high relative significance for both income and liquid assets, and, as we shall see more clearly in the detailed estimates to follow, some suggested influence of windfalls. The role of contractual savings in shaping discretionary savings will also be developed more fully below.

Estimates—Ungrouped Data

Some of the main features of the British propensity to save are evident from the analysis of grouped data. In more refined sub-groups of respondents, especially those who are extreme in some manner in their savings behaviour, we can learn more about particular factors affecting savings. Calculations from small sub-groups, though, must of necessity be made with ungrouped data. Moreover, in the second stage of analysis we want the flexibility offered by ungrouped data for studying different forms and combinations of variables. We also want computations in a form suitable for examination of the distributions of residuals.

From the 1953 main sample we have taken three significant occupational groupings—self-employed, managerial and technical, retired and unoccupied—for separate estimation of our 3-equation model of savings. At one extreme, the self-employed are high savers, and at the other extreme, the retired and unoccupied are low savers (or high dis-savers). The managerial and technical group consists to a large extent of high income respondents who, like the self-employed, may also be high savers. They form a useful comparative group alongside the self-employed to determine whether income or occupation (or both) is the significant factor influencing savings. The extreme importance of the self-employed group leads us to a second consideration of their behaviour from data collected in the 1954 Survey. This is especially important because the 1953 Survey appears to have been deficient in its sampling response from self-employed income units. Finally, to give a comprehensive picture of the whole population from ungrouped data, we have computed equations from the 1954 re-interview sample, a sub-sample of the 1953 sample interviewed again in 1954. This sample is considerably smaller than that in any of the main surveys and easily manageable for calculations from ungrouped data.

The first results are those for self-employed income units in the 1953 Survey. There were 162 respondents in this category representing 4·4 per cent. (weighted) of the total number of units in the complete sample. Self-employed units reported an average savings

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figure of £129, while that for the sample as a whole was only £4. It is easy to see the strategic importance of this group in savings behaviour.

In the calculation of the estimated model of savings, cases in which any of the variables considered were not ascertained were excluded, as were extreme cases defined as those in which the ratios S_D/Y, S_c/Y, D/Y, L/Y, or W/Y exceeded 10·0. These criteria were applied separately to each equation being estimated; therefore the same number of income units is not used for each estimated equation within an occupational sub-group.

Self-employed income units, 1953 Savings Survey—

(5.e.s.1)

$$S_D/Y = \underset{}{-1{\cdot}27} + \underset{(0{\cdot}29)}{0{\cdot}70} \log Y/N - \underset{(0{\cdot}15)}{0{\cdot}033}\,(L/Y)(Y_{-1}/Y) - \underset{(0{\cdot}023)}{0{\cdot}0062}\,A \qquad (0{\cdot}0027)$$

$$+ \underset{(0{\cdot}09)}{0{\cdot}13}\,W/Y - \underset{(0{\cdot}30)}{0{\cdot}065}\,S_c/Y$$

$$\frac{\partial S_D}{\partial Y} = 0{\cdot}36 \qquad R = 0{\cdot}55$$

(5′.e.s.1)

$$S_D/Y = \underset{}{-1{\cdot}20} + \underset{(0{\cdot}15)}{0{\cdot}70} \log Y/N - \underset{(0{\cdot}021)}{0{\cdot}022}\,(L/Y)(Y_{-1}/Y) - \underset{(0{\cdot}0028)}{0{\cdot}0065}\,A$$

$$+ \underset{(0{\cdot}09)}{0{\cdot}12}\,W/Y - \underset{(0{\cdot}40)}{0{\cdot}10}\,S_c/Y - \underset{(0{\cdot}20)}{0{\cdot}36}\,D/Y$$

$$\frac{\partial S_D}{\partial Y} = 0{\cdot}38 \qquad R = 0{\cdot}58$$

(6.e.s.1)

$$S_c/Y = 0{\cdot}073 - \underset{(0{\cdot}00083)}{0{\cdot}00065}\,A + \underset{(0{\cdot}040)}{0{\cdot}017} \log N + \underset{(0{\cdot}016)}{0{\cdot}074}\,M$$

$$\frac{\partial S_c}{\partial Y} = 0{\cdot}07 \qquad R = 0{\cdot}36$$

(7.e.s.1)

$$D/Y = 0{\cdot}077 - \underset{(0{\cdot}0013)}{0{\cdot}0018}\,A + \underset{(0{\cdot}006)}{0{\cdot}014}\,L/Y$$

$$+ \underset{(0{\cdot}033)}{0{\cdot}074}\,H + \underset{(0{\cdot}00045)}{0{\cdot}00045}\,\frac{Y - Y_{-1}}{Y_{-1}} + \underset{(0{\cdot}043)}{0{\cdot}050}\,B$$

$$\frac{\partial D}{\partial Y} = 0{\cdot}07 \qquad R = 0{\cdot}39$$

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In commenting on these results, we must first note changes in definition, notation, or form of equation. The variable D in these equations consists of expenditures on household durables for goods costing more than £25 per item and of expenditure on home decorations and repairs. In the durable goods equation, we experimented with a separate income-change variable, $Y - Y_{-1}/Y_{-1}$, which proved to be statistically unimportant, and with an indicator of marital status ($B = 1$ for income units consisting of 2 or more persons; $B = 0$ for income units consisting of 1 person) which similarly has little effect on the entire relationship. We are not, however, using mechanically the simple rule that an estimated coefficient must be some fixed multiple of its sampling error to qualify for retention in the model. Some variables for whose significance we have firm evidence from other sources or from repeated calculations elsewhere in this entire study will be retained as estimates in particular equations in which they contributed little to the overall correlation.

Current income receipts represent clearly the most important single variable in the discretionary savings of the entrepreneurial group. The marginal propensity to save, estimated at 0·36 in (5.e.s.1) shows the comparatively high gradient of the savings-income relation for this group of savers. The earlier estimates from grouped data for the sample as a whole yielded values somewhat less than 0·10. The estimates used in this comparison are computed at the point of means for the group or sample considered. The mean income level for self-employed units is higher than for the sample as a whole, and this accounts for some of the difference, but the coefficient of log Y/N is also much higher in (5.e.s.1) and this contributes significantly to the difference.

The liquid asset coefficient, though multiplied by Y_{-1}/Y, in (5.e.s.1) is not far different from the coefficient obtained from the grouped data of the whole sample. The age effect is more pronounced and definite in sign with these ungrouped data of the self-employed than with the grouped data. Windfalls do not appear to be significantly related to the discretionary savings of entrepreneurs. This conclusion is quite different with the other occupational groups, and, for the sample as a whole, does seem to be of some significance.

For these self-employed units the addition of durable goods to savings estimates adds little to the total degree of correlation. The coefficient obtained suggests less than perfect substitution between S_D and D on a £ for £ basis. It is worth noting, however, that the marginal propensity to save rises and the marginal asset effect falls as D/Y is added as an explanatory variable to the equation for S_D/Y. The results obtained are quite consistent with those in (7.e.s.1) where we find a marginal propensity to spend of about 0·07 and a marginal asset effect of about + 0·014.* In this expenditure equation, home ownership shows a high degree of relationship to expenditure, but age shows only a small amount.

In the equation for contractual savings, the only factor of much consequence is mortgage status, although the other estimates are of the anticipated sign and reasonable in magnitude.

Because of the great importance of saving among the self-employed, we made similar calculations for this occupational group from the 1954 Survey. In this survey there were 242 self-employed respondents making up 7 per cent. of the sample; thus coverage was more complete than in the previous year, giving an additional motivation to the duplicate set of computations.

* If the marginal propensity to save in (5′.e.s.1) were not reduced by the dependence of D on Y, the margin above this statistic from (5.e.s.1) would be larger.

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Self-employed income units, 1954 Savings Survey—

(5.e.s.2)

$$S_D/Y = \underset{(0\cdot16)}{-0\cdot53} + \underset{(0\cdot09)}{0\cdot38}\log Y/N - \underset{(0\cdot011)}{0\cdot022}\,(L/Y)(Y_{-1}/Y) - \underset{(0\cdot0024)}{0\cdot0052}\,A$$
$$- \underset{(0\cdot039)}{0\cdot050}\,W/Y - \underset{(0\cdot20)}{1\cdot08}\,S_c/Y$$

$$\frac{\partial S_D}{\partial Y} = 0\cdot25 \qquad R = 0\cdot62$$

(5′.e.s.2)

$$S_D/Y = \underset{(0\cdot15)}{-0\cdot56} + \underset{(0\cdot09)}{0\cdot45}\log Y/N - \underset{(0\cdot011)}{0\cdot014}\,(L/Y)(Y_{-1}/Y) - \underset{(0\cdot0023)}{0\cdot0070}\,A$$
$$- \underset{(0\cdot036)}{0\cdot062}\,W/Y - \underset{(0\cdot18)}{1\cdot13}\,S_c/Y - \underset{(0\cdot17)}{0\cdot72}\,D/Y$$

$$\frac{\partial S_D}{\partial Y} = 0\cdot28 \qquad R = 0\cdot69$$

(6.e.s.2)

$$S_c/Y = \underset{(0\cdot06)}{0\cdot12} - \underset{(0\cdot0010)}{0\cdot0019}\,A + \underset{(0\cdot049)}{0\cdot051}\log N + \underset{(0\cdot026)}{0\cdot030}\,M$$

$$\frac{\partial S_c}{\partial Y} = 0\cdot05 \qquad R = 0\cdot30$$

(7.e.s.2)

$$D/Y = \underset{(0\cdot04)}{0\cdot12} - \underset{(0\cdot0010)}{0\cdot0017}\,A + \underset{(0\cdot0070)}{0\cdot0071}\,L/Y + \underset{(0\cdot021)}{0\cdot057}\,H$$

$$\frac{\partial D}{\partial Y} = 0\cdot06 \qquad R = 0\cdot30$$

The only significant conceptual difference in comparison with the 1953 Survey data is that the variable D in 1954 includes all expenditure on durable goods costing £10 or more per item. This brings considerably more purchases into the scope of this variable. As before, expenditures on decorations and repairs are also included.

These 1954 sample estimates are similar in many respects to the previous set, especially in relative importance and direction of influence of most variables. In the equation for discretionary savings it appears that the constant term and the coefficient of log Y/N change greatly between the two years. These changes are compensatory, however, and the net results are estimated marginal propensities to save that are much more nearly the same. The marginal propensity is lower in the 1954 sample data, but the marginal propensities to save contractually or spend on durables change very little between the two. As in the 1953 estimates, income proves to be the variable most strongly related to discretionary savings. While the coefficient of windfalls changes sign, it does not seem to play a powerful role in the entrepreneurial group.

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The most important single change in the two sample estimates is in the coefficient of contractual savings. The estimate for 1954 suggests a complete £ for £ substitution between contractual and discretionary savings, and the sampling error attached to the estimated coefficient is comparatively small. This is the only case investigated in this study where the degree of substitution is clearly as high and it may well be a chance occurrence.

In both entrepreneurial samples, the addition of a durable consumption variable to the equation for discretionary savings has little effect on the overall degree of correlation. This is not generally the case for all occupation groups.

The estimated parameter differences in the equations for contractual savings and for durable consumption in the two surveys are not beyond the bounds permitted by the sampling variability involved, but it is worth noting that the asset effect on durable consumption falls markedly from 1953 to 1954. We should observe, however, the similarity in the two samples of the change in the asset effect on discretionary savings as durable consumption is added to the equation as an explanatory variable.

In these samples with the entrepreneurial sub-group, attempts were made to compute statistical estimates of equation (5′)—the discretionary savings equation with durable consumption as an explanatory variable—by using some form of equation-system method of estimation, instead of the *single-equation least-squares method.* We regarded all explanatory variables in equation (5′) as predetermined except S_D/Y and D/Y, which we assumed to be jointly subject to individual choice. We used the method of *limited-information maximum-likelihood* in the hopes of obtaining an unbiased estimate of the relationship between S_D and D. The essential point of this method involves using predetermined variables for the estimation calculations that are important in the consumer choice about durable expenditures but not directly involved in the associated saving decision. We have used such predetermined variables as H, L/Y, Y_{-1}/Y, and B, and of these it is only H which is clearly significant in equation (7) but not in (5′). The results obtained with the limited information method were not useful or revealing. They yield absurd coefficients for many of the variables.

A related method, called *the method of instrumental variables,* is suggested in this situation because there is only one variable among the set used which has a clear relationship to D/Y but not to S_D/Y. This variable is H, housing status. We applied the method of instrumental variables using H as the single instrument needed since there is only one variable not treated as predetermined in equation (5′). The results obtained were equally unrealistic. We, therefore, accepted as our best estimates those obtained from least-squares regressions, and look upon the model consisting of estimates of (5), (6), and (7) as most satisfactory. The least squares estimates of (5′), though biased, are of interest and show some degree of substitutability between S_D and D. There is probably less than £ for £ substitutability, but we are uncertain about this point.

Contractual savings do not raise the same problems as durable expenditure in our model since they are more nearly predetermined in the consumer's budget. Contractual savings are not predetermined in the first instance when an initial decision is made to enter a contract for fixed future payments; they are predetermined only after the contractual arrangement has begun to operate. We may bring this aspect of the matter more directly into the model by estimating an equation for contractual savings only of those actually committed to a contract. In applying the model to forecasting and business

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cycle analysis for the aggregate economy the relevant statistic needed to link the contractual equation for those with commitments to the same equation for all income units (our equation (6)) is the percentage of units having savings contracts. This statistic is slow moving and fairly predictable.

Our calculations of a contractual savings equation for those having contractual commitments adds little to the equation already estimated consisting of all income units, although one might have expected a higher correlation to have been obtained if all zero savers were excluded. In fact the correlation is lower by a very small amount.

$$S_c/Y = 0{\cdot}17 - 0{\cdot}0025\,A + 0{\cdot}021 \log N + 0{\cdot}032\,M$$

$$R = 0{\cdot}27$$

The constant term is greater than in (6.e.s.2), showing a higher average level of contractual savings after zero cases are omitted. None of the coefficients are significantly different from the previous estimates, but one can interpret the difference between the two constant terms ($0{\cdot}17 - 0{\cdot}12 = 0{\cdot}05$) as arithmetically reasonable.

The degree of correlation is generally higher in the equations for S_D/Y than in those for either S_c/Y or D/Y. The ratio of discretionary savings to income is strongly correlated with income; therefore contribution of a significant amount to the total correlation is assured by using income (log Y) as one of the explanatory variables. The correlation coefficient in the equations for S_c/Y and D/Y receives no contribution from a relationship to income, yet both S_c and D are significantly related to income. For self-employed units in the 1953 sample the income correlations alone are

$$r_{S_cY} = 0{\cdot}48$$

$$r_{DY} = 0{\cdot}32$$

One should not therefore be disappointed, in any sense, in the size of the correlation coefficients obtained for the ratio variables.

For an equation closely resembling, but not identical with (5.e.s.2), residual values of S_D/Y were computed for an empirical study of the nature of the distribution of the disturbance term in the equation. The formula used was

(5.e.s.3)

$$\text{Residual} = S_D/Y - 0{\cdot}38 + 27{\cdot}36\,N/Y + 0{\cdot}029\,(L/Y)(Y_{-1}/Y)$$

$$+ 0{\cdot}0026\,A + 1{\cdot}02\,S_c/Y$$

This equation differs from the ultimate choice in (5.e.s.2) in that the reciprocal instead of the logarithm of Y/N is used as an explanatory variable and that windfalls, W/Y, were not used in the preliminary set of computations.

Two characteristics of the distribution of residuals were closely examined: (1) the uniformity of variance among different income classes (homoscedasticity); and (2) the normality of the distribution. In Table 1 the variances are given by income classes for (5.e.s.3) together with similar calculations for (6.e.s.2) and (7.e.s.2), the contractual savings and durable consumption equations.

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TABLE 1

Variance of Residuals of Savings and Expenditure Functions by Income Class

Self-Employed Income Units, 1954

Net Income (£)	(5.e.s.3)	(6.e.s.2)	(7.e.s.2)
100–199	·0185	·0010	·0335
200–299	·0273	·0023	·0044
300–399	·0330	·0832	·0041
400–499	·0212	·0032	·0095
500–599	·0298	·0021	·0077
600–699	·0556	·0022	·0085
700–799	·0231	·0012	·0028
800–899	·0341	·0017	·0031
900–999	·0341	·0070	·0209
1,000 and over	·0724	·0038	·0114

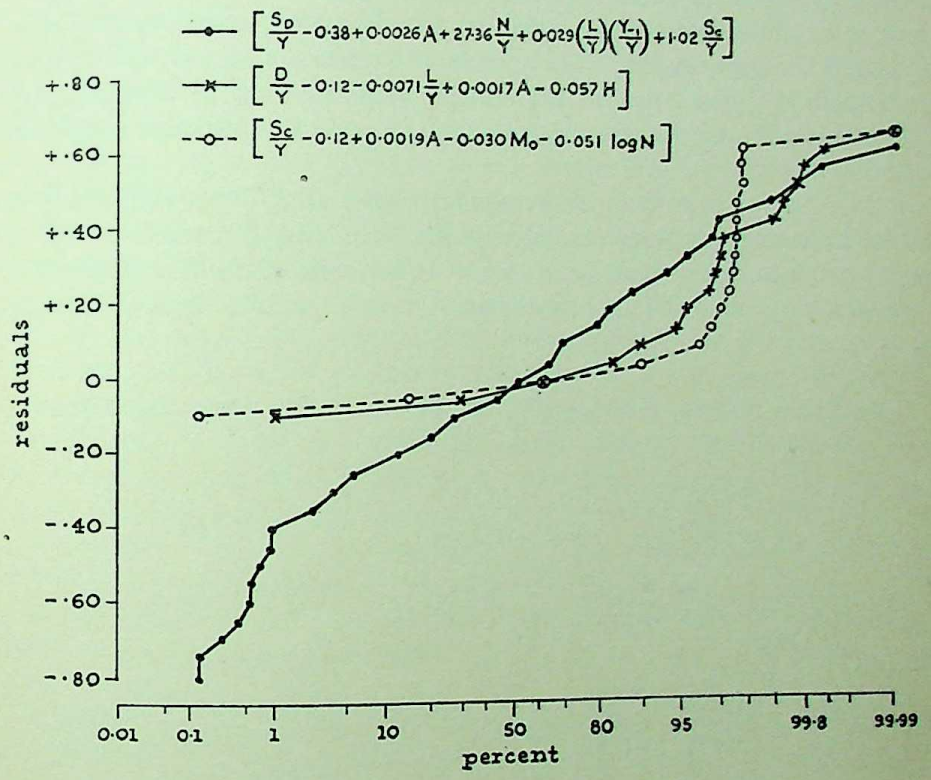

FIG. 4.—Cumulative distributions of residuals.

Having excluded extreme observations—cases in which S_D/Y and L/Y exceeded 10·0—we do not even find the usual tendency for the variances to be high in the lowest income classes except in the case of durable consumption. There is some evidence of a large variance in the top income class for discretionary savings and in the top two classes for durable consumption but not in the case of contractual savings. In general, the strong patterns of heteroscedasticity found in untransformed data from cross-section samples have been eliminated. It is quite safe to base computational methods on the assumption of uniform variance after having transformed to ratio variables.

The normality of residuals is less apparent. The simplest way of testing the normality of distribution is to plot the cumulative frequency function of residuals on probability

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paper. In Fig. 4 we find nearly a straight line and close conformity with the normal hypothesis for the discretionary savings function. The departures from normality are obvious for the other two sets of residuals. Both of these are influenced by the fact that the dependent variable is non-negative and densely massed at zero.

Managerial and technical income units, 1953 Savings Survey.—We continue with the occupational decomposition of the 1953 Survey for the study of savings behaviour. The self-employed group is taken as an extreme because they are high savers on two accounts. They save much because of the nature of their occupation—to meet their heavy capital requirements—and because they are frequently in the upper income classes. The managerial and technical groups provide an interesting comparison, because these units are similarly placed on the income scale although they do not have the problem of finding the necessary capital from their own resources alone for the financing of their enterprises. Many of these income units are really in the same position as the self-employed in every respect but legal form of organization. The owners and directors of private companies are business men in the same sense as the self-employed, but from an accounting point of view they are treated as salaried employees, and their company savings are not reckoned as personal savings.

In the 1953 Survey there were 254 respondents comprising 7·3 (weighted) per cent. of the sample in the managerial and technical group. Exclusive of cases in which variables were not ascertained or in which there were extreme values of S_D/Y or L/Y (in excess of 10·0), we used these individual observations to estimate the following equations:

(5.e.m.1)

$$S_D/Y = \underset{(0\cdot07)}{-\,0\cdot29} + \underset{(0\cdot03)}{0\cdot15}\log Y/N - \underset{(0\cdot0054)}{0\cdot0015}\,(L/Y)(Y_{-1}/Y) - \underset{(0\cdot0005)}{0\cdot0023}\,A$$
$$- \underset{(0\cdot007)}{0\cdot043}\,W/Y - \underset{(0\cdot11)}{0\cdot31}\,S_c/Y$$
$$\frac{\partial S_D}{\partial Y} = 0\cdot03 \qquad R = 0\cdot18$$

(5′.e.m.1)

$$S_D/Y = \underset{(0\cdot17)}{-\,0\cdot33} + \underset{(0\cdot07)}{0\cdot19}\log Y/N + \underset{(0\cdot01281)}{0\cdot00027}\,(L/Y)(Y_{-1}/Y) - \underset{(0\cdot0014)}{0\cdot0020}\,A$$
$$- \underset{(0\cdot018)}{0\cdot015}\,W/Y - \underset{(0\cdot26)}{0\cdot32}\,S_c/Y - \underset{(0\cdot09)}{0\cdot76}\,D/Y$$
$$\frac{\partial S_D}{\partial Y} = 0\cdot04 \qquad R = 0\cdot63$$

(6.e.m.1)

$$S_c/Y = \underset{(0\cdot016)}{0\cdot054} + \underset{(0\cdot0003184)}{0\cdot0000028}\,A - \underset{(0\cdot0177)}{0\cdot0019}\log N + \underset{(0\cdot010)}{0\cdot066}\,M$$
$$\frac{\partial S_c}{\partial Y} = 0\cdot07 \qquad R = 0\cdot52$$

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(7.e.m.1)

$$D/Y = \underset{(0\cdot056)}{0\cdot074} + \underset{(0\cdot00132)}{0\cdot00045}\,A + \underset{(0\cdot0133)}{0\cdot0028}\,L/Y + \underset{(0\cdot033)}{0\cdot045}\,H$$

$$\frac{\partial D}{\partial Y} = 0\cdot11 \qquad R = 0\cdot13$$

The savings-income relation is decidedly less steep for managers than for self-employed. This point comes out most clearly in the equation for discretionary savings where the estimated marginal propensity to save is much less than in (5.e.s.1). Without the assistance of the durable goods variable, D/Y, the equation for discretionary savings shows low correlation and all the coefficients except that of S_c/Y are less in absolute value than the corresponding estimates for the self-employed. In both samples of self-employed, durable expenditures are not as closely related to S_D/Y as is the combination of the other variables. In the cases of contractual savings and durable expenditures, the marginal propensities are comparable with those estimated for self-employed. The correlation in the contractual savings equation is as high as 0·52 only because of the effect of the mortgage variable. There is almost no correlation in the durable goods equation, and we note especially the insignificance of the liquid asset variable.

The self-employed groups are the strategic people for the study of savings and as we move along the occupational classification to the next group which most resembles the self-employed we find substantial changes in behaviour.

Retired and unoccupied savings units, 1953 Savings Survey.—At the other end of the occupational scale, we have the group least inclined towards saving for the future, the retired and unoccupied units. These people are mostly elderly couples, widows, widowers, spinsters and bachelors. They are often living on modest pensions, and those who have capital are usually drawing on it. Their saving on balance is negative and would be even more so if we treated the receipt of annuity funds as partial capital decumulation instead of purely as income. The group is large, being 22·8 per cent. (weighted) of the sample, and quite mixed as regards income, wealth, and social status.

The equations estimated from the 471 cases in this group with the same types of exclusions as in the other groups are given below.

(5.e.r.1)

$$S_D/Y = \underset{(0\cdot143)}{-\,0\cdot026} - \underset{(0\cdot061)}{0\cdot049}\log Y/N - \underset{(0\cdot009)}{0\cdot030}\,(L/Y)(Y_{-1}/Y)$$
$$+ \underset{(0\cdot0010)}{0\cdot0011}\,A - \underset{(0\cdot002)}{0\cdot081}\,W/Y - \underset{(0\cdot28)}{0\cdot12}\,S_c/Y$$

$$\frac{\partial S_D}{\partial Y} \leqslant 0 \qquad R = 0\cdot24$$

(5′.e.r.1)

$$S_D/Y = \underset{(0\cdot131)}{-\,0\cdot076} + \underset{(0\cdot0572)}{0\cdot0036}\log Y/N - \underset{(0\cdot009)}{0\cdot022}\,(L/Y)(Y_{-1}/Y)$$
$$+ \underset{(0\cdot00090)}{0\cdot00041}\,A - \underset{(0\cdot015)}{0\cdot084}\,W/Y + \underset{(0\cdot25)}{0\cdot16}\,S_c/Y - \underset{(0\cdot11)}{0\cdot81}\,D/Y$$

$$\frac{\partial S_D}{\partial Y} \leqslant 0 \qquad R = 0\cdot45$$

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(6.e.r.1)

$$S_c/Y = \underset{}{0{\cdot}026} - \underset{(0{\cdot}00016)}{0{\cdot}00013}\,A + \underset{(0{\cdot}0121)}{0{\cdot}0056}\log N + \underset{(0{\cdot}01)}{0{\cdot}13}\,M$$
$$(0{\cdot}011)$$

$$\frac{\partial S_c}{\partial Y} = 0{\cdot}03 \qquad R = 0{\cdot}64$$

(7.e.r.1)

$$D/Y = \underset{}{0{\cdot}071} - \underset{(0{\cdot}00045)}{0{\cdot}00098}\,A + \underset{(0{\cdot}0037)}{0{\cdot}0064}\,L/Y + \underset{(0{\cdot}015)}{0{\cdot}097}\,H + \underset{(0{\cdot}0073)}{0{\cdot}0051}\,W/Y$$
$$(0{\cdot}030)$$

$$\frac{\partial D}{\partial Y} = 0{\cdot}03 \qquad R = 0{\cdot}40$$

These results are quite different from those found for the other groups investigated and call for careful appraisal and comment. Throughout all forms of savings, these units are not responsive to changes in income level. The marginal propensity to save in a discretionary form is practically *nil* and even negative at the point values for sample averages. Most of the income units, as remarked previously, are elderly, and age effects within the group are probably beyond the threshold of significance.

Fisher (1956), in fitting a purely linear form of the savings function and not distinguishing between contractual and discretionary savings, estimates negative coefficients of income in several sub-groupings of the 1953 sample. He finds negative coefficients mainly among very old, very young or retired and unoccupied income units. To some extent, his sprinkling of negative coefficients may be ascribed to sampling fluctuations, but the systematic appearance of the inverse relationship between savings and income in the particular age and occupation groups mentioned above is hardly a purely chance event. Friedman's (1957) highly specific theory of savings is unable to cope with such phenomena; therefore he argues, in a comment on Fisher's paper, that either these findings are a consequence of pooling primary and secondary income units together or the data are wrong. In fact, neither explanation offered by Friedman seems to be adequate. In separate tabulations for primary and secondary units, as outlined by Friedman, in age and occupation groups, we still find negative correlations between savings and income among very young and very old income units. Friedman's charge about the incorrectness of the data, is, of course, a court of last resort when theory is not substantiated by fact. There is, however, a sensible explanation of the inverse relationship for the aged income units, in which class we find most of the retired and unoccupied units.

The post-war inflation and death duties are two powerful deterrents to savings by older income units. In a separate experimental survey concentrating on the upper income groups we have particularly found this negative attitude towards saving expressed by elderly people (Klein, *et al.*, 1956). At the upper end of the income range, we find people who are strongly bent on maintaining past expenditure standards. Attempts to preserve, as far as possible, the "middle class way of life" in post-war Britain have necessitated large expenditures in relation to income, probably even total expenditures in excess of income for a large number of elderly persons living on rentier income. Both inflation and steeply progressive income taxes have contributed to this situation, given that people are reluctant to change their way of living. With high death duties, elderly people are

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quite prepared to consume capital while they are still living. Because of the progressive rates of income tax and because attachment to past standards is likely to be more important the richer one is, it is not surprising to find a negative relation between saving and income in the retired and unoccupied group (5.e.r.1). In the lower income groups, people's attitudes towards saving may be similar, but it would not matter in any case since these income units spend practically the whole of a meagre income and have little scope for variation of either saving or spending. Practically non-existent savings at low levels of income coupled with a tendency towards depletion of capital in modern circumstances among the upper income groups will, in itself, produce a negative correlation on balance.

Contractual savings are not frequent among this group, the amount being *nil* for 45 per cent. of them; hence we find only an insignificant coefficient of S_c/Y in the discretionary savings equation.

In contrast to the lack of response to differences in income level, there is a noticeable response to differences in wealth level. Liquid assets are of some importance in the equations for discretionary savings and durable expenditures. Of even more significance is the effect of windfalls—transfers of capital. These units have a definite negative association between savings and windfall receipts. The windfall variable was added to the durable goods equation, but it does not appear that these units spend on durables when they spend their windfalls.

Not many of the retired and unoccupied income units buy durable goods, but when they do buy, such purchases have a sharp effect on savings. They are not likely to buy motor cars or motor cycles; therefore we find a strong gradient attached to the home ownership indicator in the durable goods equation. They are similarly not likely to be paying life assurance premiums or contributing to superannuation schemes; hence their main form of contractual savings is by mortgage repayments, although many elderly people in this group have already completed the retirement of mortgage debt. The coefficient of M in equation (6.e.r.1) is quite significant.

These calculations are mainly useful in showing the wide variety of personal situations and the very different patterns of savings behaviour involved. For these reasons, highly simplified and uniform treatments for different groups are ruled out. The savings process is multivariate to a high degree, and the strategic factors in the different situations must be individually examined.

The re-interview sample of 1954.—To give a frame of reference to the various estimates for occupational sub-groups in the 1953 and 1954 Surveys, we have computed coefficients for the same model from a sample including all occupation groups in the 1954 Re-interview Survey. This sample consists of all respondents with incomes of £800 or more in the 1953 Survey and one-fifth of these with incomes below £800. Of course, all respondents chosen in the sample did not co-operate and some were unavailable. In the final tabulations we find 429 successful interviews. This comparatively small sample provides a useful frame of reference for the preceding estimates, however, because it includes aspects of both 1953 and 1954. It also enables us to check more carefully on the effects of income change since we have two separate reports of income, one in the 1953 interview and one in the 1954 interview. In a similar way it enables us to get a better estimate of beginning-of-year liquid assets by using values from the first interview.

In addition to supposed improvements in the matter of respondent memory, we find another advantage of the re-interview survey to be the possibility of associating expressed

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subjective feelings or expectations at a prior time point (1953 Survey) with subsequent behaviour (1954 Survey) (Klein, 1954; Klein and Lansing, 1955). Such associations have obvious applications in economic forecasting, but in a more fundamental sense they enable us to make some rough tests of hypotheses about expectations in economics. Without the re-interview tool these tests could not be satisfactorily applied because *ex post* expectations may be strongly influenced by the actual consumer decisions taken.

The 1954 Re-interview Survey has a combination of the biases and errors in the 1953 and 1954 main surveys. It has the sampling biases of the 1953 Survey since it consists wholly of 1953 respondents. It has both sampling and interviewing errors parallel to those occurring in the 1954 Survey (Klein and Vandome, 1957). Moreover, as a re-interview survey, it has its own peculiar biases. For obvious reasons there are too few young people, secondary units, and very aged people in the sample.

If these particular biases are strong enough, they will lead to a higher propensity to save in the re-interview than in the main sample because the young and the aged are both low savers. By making some separate tabulations excluding the retired and unoccupied units, we can obtain estimates of a sub-population free of one of the main sources of sample bias. We have not similarly excluded the youngest income units from our calculations.

First we present estimates for the entire re-interview sample, excluding cases in which variables are not ascertained or in which S_D/Y or L/Y exceed 10·0 in absolute value.

(5.e.R.1)

$$S_D/Y = \underset{(0\cdot11)}{-0\cdot21} + \underset{(0\cdot04)}{0\cdot13}\log Y/N - \underset{(0\cdot012)}{0\cdot018}\,(L/Y)(Y_{-1}/Y) - \underset{(0\cdot0007)}{0\cdot0015}\,A$$

$$- \underset{(0\cdot03)}{0\cdot13}\,W/Y - \underset{(0\cdot25)}{0\cdot57}\,S_c/Y$$

$$\frac{\partial S_D}{\partial Y} = 0\cdot06 \qquad R = 0\cdot70$$

(6.e.R.1)

$$S_c/Y = \underset{(0\cdot008)}{0\cdot039} - \underset{(0\cdot00015)}{0\cdot00027}\,A + \underset{(0\cdot0099)}{0\cdot0058}\log N + \underset{(0\cdot007)}{0\cdot075}\,M$$

$$\frac{\partial S_c}{\partial Y} = 0\cdot04 \qquad R = 0\cdot65$$

(7.e.R.1)

$$D/Y = \underset{(0\cdot036)}{0\cdot086} - \underset{(0\cdot00040)}{0\cdot00092}\,A + \underset{(0\cdot0095)}{0\cdot0058}\,L/Y + \underset{(0\cdot015)}{0\cdot052}\,H$$

$$\frac{\partial D}{\partial Y} = 0\cdot06 \qquad R = 0\cdot27$$

These results are not far, sampling errors considered, from the estimates obtained from the grouped data of the 1953–54 Surveys. They are also similar, in some respects, to the estimates within the self-employed group for the Surveys of 1953 and 1954, except that the marginal coefficients are frequently larger in absolute value for the latter. In the contractual savings and durable expenditures equations, the mortgage and home owner-

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ship variables are the most significant. The marginal propensity to save in a discretionary form is far lower than for self-employed units and far higher than for retired and unoccupied units. Managers and technical units are not widely different from these estimates for the sample as a whole.

Having the exceptional re-interview data on income change, we examined the relationship of this variable to purchases of durable goods. To equation (7.e.R.1) we added the variable Y_{-1}/Y. At the same time $\log N$ was included as a variable to see whether family size is of any significance. As can be seen by the estimated equation below, all the previous coefficients are imperceptibly influenced by these two new variables, both of which have small coefficients in relation to their sampling errors, and there is no effect on the size of the correlation coefficient.

$$\underset{}{D/Y} = \underset{}{0{\cdot}086} - \underset{(0{\cdot}00045)}{0{\cdot}00099}\,A + \underset{(0{\cdot}0094)}{0{\cdot}0053}\,L/Y + \underset{(0{\cdot}015)}{0{\cdot}053}\,H + \underset{(0{\cdot}0305)}{0{\cdot}0085}\,Y_{-1}/Y$$

$$- \underset{(0{\cdot}060)}{0{\cdot}013}\log N \qquad R = 0{\cdot}27$$

A more homogeneous group in this sample is that exclusive of the two extremes—self-employed and retired and unoccupied. The centre group consists of all active employees in the sample and provides a set of estimates complementary to those made for extreme occupational sub-groups in preceding sections. The sample is too small to permit separate estimates for self-employed or retired and unoccupied income units.

The estimates of our model for all employed income units exclusive of N.A.'s and those with extreme values for S_D/Y and L/Y are as follows:

(5.e.R.2)

$$\underset{}{S_D/Y} = \underset{(0{\cdot}11)}{-0{\cdot}17} + \underset{(0{\cdot}044)}{0{\cdot}088}\log Y/N + \underset{(0{\cdot}01556)}{0{\cdot}00050}\,(L/Y)(Y_{-1}/Y)$$

$$- \underset{(0{\cdot}00080)}{0{\cdot}00061}\,A - \underset{(0{\cdot}061)}{0{\cdot}086}\,W/Y - \underset{(0{\cdot}24)}{0{\cdot}27}\,S_c/Y$$

$$\frac{\partial S_D}{\partial Y} = 0{\cdot}03 \qquad R = 0{\cdot}22$$

(6.e.R.2)

$$\underset{}{S_c/Y} = \underset{(0{\cdot}009)}{0{\cdot}035} - \underset{(0{\cdot}00020)}{0{\cdot}00016}\,A + \underset{(0{\cdot}0105)}{0{\cdot}0029}\log N + \underset{(0{\cdot}007)}{0{\cdot}065}\,M$$

$$\frac{\partial S_c}{\partial Y} = 0{\cdot}04 \qquad R = 0{\cdot}62$$

(7.e.R.2)

$$\underset{}{D/Y} = \underset{(0{\cdot}027)}{0{\cdot}094} - \underset{(0{\cdot}0007)}{0{\cdot}0011}\,A + \underset{(0{\cdot}013)}{0{\cdot}024}\,L/Y + \underset{(0{\cdot}020)}{0{\cdot}040}\,H$$

$$\frac{\partial D}{\partial Y} = 0{\cdot}06 \qquad R = 0{\cdot}23$$

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The equation for discretionary savings differs markedly from (5.e.R.1), but the other two estimates are not much different from those for the entire re-interview sample. In the first of the three equations, we note the drop in both the marginal propensity to save and in the degree of correlation. Having withdrawn the retired and unoccupied income units for this second set of calculations, we find a decrease in the estimated coefficient of windfalls and a growth in its associated sampling error.

The other type of information peculiar to the re-interview sample is that, mentioned earlier, on advance indicators of subjective magnitudes from the first of the two surveys.

The two principal subjective variables considered are

$I = 2$ if expects income increase, 1953 Survey,
$= 1$ if expects no income change, 1953 Survey,
$= 0$ if expects income decrease, 1953 Survey,

$B = 2$ if feeling better off financially, 1953 Survey,
$= 1$ if feeling neither better nor worse off financially, 1953 Survey,
$= 0$ if feeling worse off financially, 1953 Survey.

The expression of feeling "better" or "worse off" is quite different from either past or expected income change. In reading respondents' reasons for saying that they feel better or worse off, we find that they are taking a whole range of financial considerations into account—whether they have had recent good fortune, whether they have managed to save in the past, whether the cost-of-living has been encroaching upon their earnings, whether their family commitments have changed etc.

For the group of all employees, excluding cases in which any variable used is not ascertained, we estimate

$$S_D/Y = \underset{(0\cdot10)}{-0\cdot14} + \underset{(0\cdot041)}{0\cdot054}\log Y/N + \underset{(0\cdot018)}{0\cdot024}\,(L/Y)(Y_{-1}/Y) - \underset{(0\cdot00089)}{0\cdot00034}\,A$$

$$- \underset{(0\cdot23)}{0\cdot35}\,S_c/Y + \underset{(0\cdot020)}{0\cdot012}\,I + \underset{(0\cdot0168)}{0\cdot0094}\,B \qquad R = 0\cdot33$$

The two subjective variables contribute little to the relationship. Their coefficients have large sampling errors and have little influence on the coefficients of the other variables. These are hardly affected if the equation is re-estimated without I and B.* One would probably expect the coefficient of I to be negative. Anticipated decreases in income make savings urgent, and increases permit financial relaxation.

Another method of looking for the possible effect of subjective feelings on savings behaviour is through the classification of residuals from the estimated savings equations into different groups of the I- and B-variables. For this computation, we have used the residuals from (5.e.R.1), the discretionary savings equations for the whole re-interview sample including employees, self-employed, and the retired and unoccupied. Some of these results are suggestive but not strong enough to be considered statistically significant.

* This remark does not refer to (5.e.R.2) because there are several cases for which I and B are not ascertained in that estimated equation.

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TABLE 2

Residuals from (5.e.R.1) *by Classes of Income Expectation and Financial Well-being*

Re-interview Survey, 1954

	Frequency	*Mean Residual*
Income expectations (early 1953)—		
Increase	88	·0055
No change	231	—·0060
Decrease	39	·0232
N.A.	29	·0179
Financial well-being (early 1953)—		
Feeling better off	110	·0165
About same	162	—·0211
Feeling worse off	59	·0081
N.A.	56	·0309

In each case, looking at the extreme groupings, we find differences of some interest though moderate in size. Those expecting a decrease saved more on balance than did those expecting an increase. This type of behaviour would be sensible as a provision for expected financial deterioration. In the second part of the table we see that those feeling better off tend to save more than those feeling worse off. In view of the reasons given by respondents for an expression of feeling better or worse off (see p. 86), a tendency for feeling better off to be associated with larger residuals is quite reasonable.

The weakness of these findings prompts us to look somewhat deeper into the effects of the attitudinal variables. No single subjective expression gives adequate representation of the consumer's outlook, but perhaps a combination of several such variables will be associated with more distinguishable differences in behaviour. Accordingly we have classified re-interview respondents into four groups:

\+ Those favourable or neutral with respect to income change, 1952–53, income change 1953–54, feeling of financial well-being, 1953, expectation of financial well-being during 1954, expectation of income change during 1954.

0 Those neutral in all five categories.

? Those both favourable and unfavourable.

— Those unfavourable or neutral in all five categories.

These are mutually exclusive classes into which each income unit can be placed, not ascertained cases being classed as neutral. Four of the five variables being considered are determined in advance from the first of the two surveys. Income Change 1953–54 is ascertained on the second survey, but is included as an important dynamic factor. The list thus includes more than subjective attitudes; it includes two measures of actual performance, income change 1952–53 and 1953–54. These latter two variables, like the subjective expressions of opinion are often thought to be of significance in the explanation of consumer behaviour, yet they constantly show weak or mild association in our multivariate regressions; therefore we are combining them with the attitudinal variables into a composite which may be of some significance for short-run dynamics.

We have associated this combined classification scheme with percentages of net income spent on durables or saved.* In these tabulations we do not simultaneously take into

* For these tabulations durable expenditures do not include home repairs and decorations. Savings include both contractual and discretionary savings.

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account income, age, asset, family size, and similar differences among the several classes of the variables being considered. As for the percentage of income spent on durables; we effectively have no relationship with income, which is perhaps the main variable differing

TABLE 3

Mean Percentages of Net Income Spent on Durables or Saved by Classes of Combined Dynamic Variables

Re-interview Survey, 1954

Combined Variable Classification	*Frequency*	*Percentage of Net Income:* *Spent on Durables*	*Saved*	*Mean Net Income (£)*
+	138	6·04	2·75	504
0	14	*nil*	1·89	197
?	200	5·98	−2·39	484
−	77	3·57	−1·93	295

$$\left[\frac{S_D}{Y} + 0{\cdot}21 - 0{\cdot}13 \log\frac{Y}{N} + 0{\cdot}018\left(\frac{L}{Y}\right)\left(\frac{Y_{-1}}{Y}\right) + 0{\cdot}0015A + 0{\cdot}13\frac{W}{Y} + 0{\cdot}57\frac{S_C}{Y}\right]$$

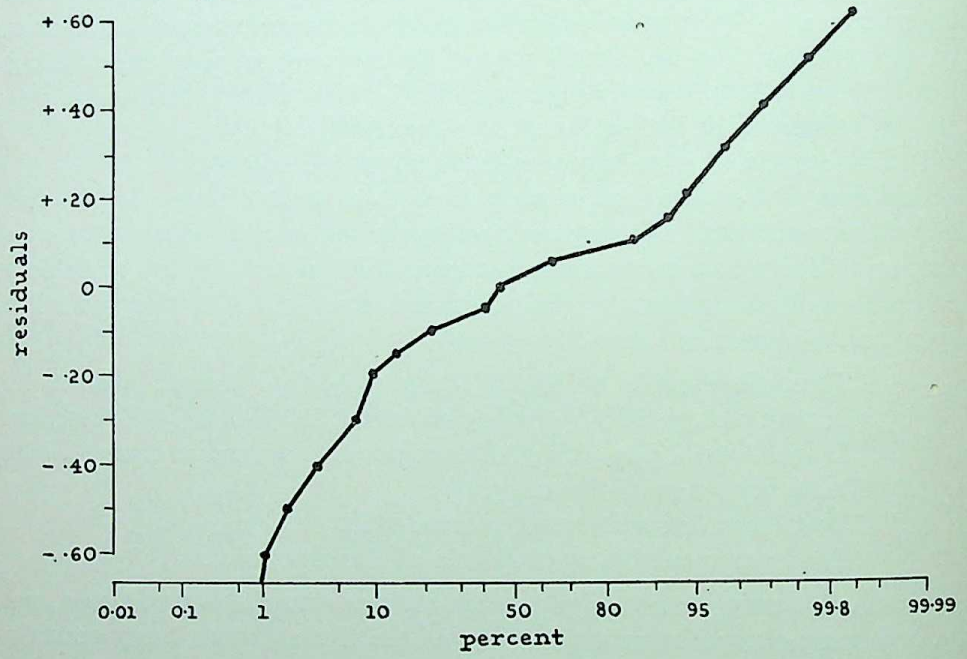

FIG. 5.—Cumulative distribution of residuals—equation (5 e.R.1).

among the classifications used. These are larger differences than found previously or than would be implied by the coefficients found for individual income change variables. The differences between the two extreme groups (+ and −) in durable expenditure is more noteworthy than the savings differences since the income differences between these two classes are responsible for some of the latter.

One final tabulation of the savings data in the re-interview Survey remains to be presented, namely the distribution of residuals from equation (5.e.R.1). Since these residuals were computed for the purpose of averaging them within classifications of income expectations and feeling of financial well-being (Table 2), they are readily available for examination

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of their distributional properties. In contrast with the residuals for the self-employed group in the 1954 Survey, these residuals do not adhere closely to the normal distribution. The line graphed in Fig. 5 departs markedly from linearity (compare Fig. 4). The residuals do form a fairly symmetrical frequency distribution. The principal departure from a normal frequency function is in their excessive peakedness—*leptokurtosis*. This is not an infrequent type of distribution met with in sample survey data for consumers (Morgan, 1954; Klein, 1954).

Summary of Results with British Survey Data

The reader may find a tabular summary convenient at this point. The marginal propensities estimated from the various samples and sub-groupings of the Oxford Savings Surveys are presented in Table 4. Some experimental calculations as well as those savings functions which include D/Y as an explanatory variable are omitted.

TABLE 4

Marginal Propensities to Save or Spend

Survey and Subgroup	*Dependent Variable*	*Marginal Propensity (at Point of Sample or Group Means)*
Grouped Data		
1953–54 whole sample*	S_D/Y	·09
1953 whole sample	S_c/Y	·04
1953 whole sample	D/Y	·01
1953 whole sample	S_D/Y	$\cdot 10 - \cdot 80 \frac{\partial S_c}{\partial Y}$
Ungrouped Data		
1953, self-employed	S_D/Y	·36
1953, self-employed	S_c/Y	·07
1953, self-employed	D/Y	·07
1954, self-employed	S_D/Y	·25
1954, self-employed	S_c/Y	·05
1954, self-employed	D/Y	·06
1953, managerial and technical	S_D/Y	·03
1953, managerial and technical	S_c/Y	·07
1953, managerial and technical	D/Y	·11
1953, retired and unoccupied	S_D/Y	Negative
1953, retired and unoccupied	S_c/Y	·03
1953, retired and unoccupied	D/Y	·03
1954, whole re-interview sample	S_D/Y	·06
1954, whole re-interview sample	S_c/Y	·04
1954, whole re-interview sample	D/Y	·06
1954, re-interview employees	S_D/Y	·03
1954, re-interview employees	S_c/Y	·04
1954, re-interview employees	D/Y	·06

* Two low income groups excluded.

A Comparison with the American Propensity to Save

The research carried out with the British data from the Oxford Savings Surveys closely follows that done in connection with the American Surveys of Consumer Finances. Indeed, the survey designs of the two field investigations are basically the same and highly comparable. Equations similar to those estimated in this paper have previously been estimated for the U.S.A.; we therefore look for a set of findings that might typify Western capitalist economies in general.

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First, let us set out in general terms some of the main points of comparison—either similarities or differences. The savings-income relationship found in the Surveys of Consumer Finances is broadly similar to that found in the Oxford Savings Survey. Savings grow with income at an increasing rate. In low income groups there is negative saving for both countries. International comparisons involve thorny questions, but some statistics that avoid absolute comparisons of money values may be revealing. In the American surveys (1948–51), the consumer reaches the "break-even" point, i.e. the point of zero savings, well before either the median or mean disposable income. The British income unit breaks even between the median and mean. On the basis of the simple relationship between savings and income without taking other variables into account, the American consumer appears to have a higher propensity to save.

Lydall finds that the discrepancy between American and British savings-income patterns is much reduced, although American savings superiority is not eliminated if tax and national insurance (U.K.) or social security (U.S.) payments are added to personal savings (Lydall, 1955). The logic of this addition is that many forms of "collective savings" are covered by these contributions and that they displace personal savings. People save for retirement in old age, the raising of children, medical contingencies and similar basic family needs which are covered in many cases by social insurance and tax funds. Payments into such funds are a crude measure of "collective savings", but in the present context they are suggestive of an explanation of the international differences. A more correct measure is subjective in nature. It would be an estimate of the amount of private savings that people no longer need to make individually because they are covered on comprehensive community schemes for such contingencies as sickness, unemployment, old age, etc.

In the U.S. there is a certain amount of "collective saving" through Social Security and Unemployment Security systems. There is no National Health Service. On the whole it is safe to say that social forms of saving are much more widespread in the U.K. The influence of this point on the comparative propensities to save has a bearing apart from information contained in sample surveys. For further analysis of the problem we must probe into the sample findings.

Not only are the curvature and general shape of the average relationship between savings and income the same in the two countries, but we also find the same type of heteroscedasticity—more variable savings at high than at low income levels.

The occupational differentials in the propensity to save are similar in Britain and in America in the sense that the self-employed have a much higher and steeper savings-income curve than do other groups. In both countries, this group dominates the statistics of personal savings. A difference in degree is worth mentioning, however. In the U.S. there is a large number of self-employed farmers as well as self-employed business men. The total group of self-employed in America is a much larger percentage of all consumer units than in Britain. This probably accounts in part for the relatively high American propensity to save. At the other end of the occupational scale for savings, we have the retired and unoccupied, a more numerous group in terms of relative frequency in Britain than in America. In both countries, these units are the low savers, absolutely and in relation to income.

The same group of variables, in addition to income level, has been considered in the calculation of savings functions for both countries. The most important economic variable

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besides income in the U.S. surveys was liquid asset holdings. A negative association between savings and liquid assets is found in the British as well as in the American data, but the marginal coefficient is considerably lower in this country. An interaction effect between liquid assets and income (sometimes between liquid assets and income change) on savings was found and stressed in the studies with the American material. In the present investigation more attention was paid to the interaction between liquid assets and income change, this type being considered superior on *a priori* grounds, but the coefficients estimated are surrounded by much uncertainty.

One is led by *a priori* considerations to regard income change as a key *dynamic* factor in consumer behaviour. We find equally disappointing results in American and British Survey data, as far as establishing the magnitude of an income change effect is concerned. One important point, however, in this connection is the apparent greater *variability* in American than in British personal incomes during recent years. To cite one revealing statistic, we find that nearly twice as many British as American consumer units had steady incomes (annual change between + 4 and — 4 per cent.) during the years 1951–54. Friedman and others laying stress on the variability of income as a basic factor in annual savings behaviour would cite this difference as evidence supporting their hypotheses since the country with the higher degree of income variability is the country with the greater propensity to save. In this paper, we note this interesting difference as a factor of great relevance but far from containing the whole of the explanation of the international differences between savings propensities.

We find important differences in age for the two countries. In both cases, we have strong convictions that age is quite important in saving behaviour, and in both cases age coefficients tend to have large sampling errors though being persistent among population sub-groups or repeated samples in their general direction of effect. It is in the direction of effect that the main difference between countries occurs. On balance, savings are positively correlated with age in the U.S. and negatively correlated with age in the U.K. Both sets of survey statistics exhibit a non-linear relation between savings and age which tends to be straightened out by transformation to the savings-income ratio. Savings rise from negative levels in the youngest groups to peak savings in middle age and fall to negative levels again in the eldest groups. Wih this basic non-linear tendency, a linear age coefficient depends on the underlying age distribution, and the function is directed either negatively or positively according to the position of dominant age groups.* The equation discussed earlier showed the net savings-age correlation to be negative in most cases. In contrast it is positive in the equations based on American data. The tendency towards dis-saving by older persons and the heavier concentration in older age groups in Britain accounts for much of this difference. The median age is lower by 5 years in America than here. This difference in the age distribution carries over as well to the occupational distribution, where we find a larger percentage of British units classified as retired and unoccupied.

Two structural differences in the countries, the older age concentration in Britain and the larger number of self-employed in America, are responsible to a large extent for the differences observed in savings behaviour. Their general effect is to make the propensity to save in private forms lower in Great Britain than in the U.S.

* Experimental computations have been made with both British and American data to establish non-linear age coefficients but the results are equally valueless in each case.

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Apart from these dominant variables and associated structural characteristics, there is a "second line" of variables related to savings. Family size has a similar effect on savings in both countries. Using either the logarithm of *per capita* income or the logarithm of the number of persons, we find a negative non-linear relationship to family size. In both cases *contractual* savings, at the jump between single and multi-person units, increases. Home ownership in this country and America is associated with higher savings rates than are found among renters and other non-home owners. In the models studied earlier in this paper, the effect of home ownership has been more closely specified as the influence of fixed mortgage obligations in contractual savings. In the American computations, we had not formed as detailed a model and simply noted the relation between total personal savings and home ownership. Though it seemed plausible to find an effect of home ownership on durable consumption expenditures, when we made the computations from the American data we found no such relationship. From the material in the Oxford Savings Survey, we do find a fairly definite positive relation between home ownership and durable consumption.* Possibly, here we have a real international difference, durable consumption being more closely tied to items used in connection with the home in Great Britain and being less dominated by the motor car as in America.

Contractual savings were not split from total savings when the equations were estimated from the American data; therefore the question of substitution between discretionary and contractual savings was not examined in the same detail as with the British data. However, in some other calculations based on an analysis of variance, Morgan (1954) found with the U.S. data a positive relation between total and contractual savings. The results of the variance analysis plus an examination of sample averages in different savings classes, suggest less than perfect substitutability between the two forms of savings. In this respect there are no striking international differences.

The Oxford Savings Survey and the Surveys of Consumer Finances are so nearly similar, that even the accounting treatment of most components of savings are identical. An especially important matter is the treatment of capital transfers or "windfalls" in the estimation of personal savings for each unit in the survey. In both surveys the receipts of windfalls are entered as negative savings and often depress, in a numerical sense, the income of each recipient unless offset by an increase in some item of positive savings.† To the extent that consumers tend to spend windfalls, on either durables or non-durables, we find a negative correlation with savings. This is the case in both British and American surveys.

In recent years, the American Surveys of Consumer Finances have become more surveys of consumer attitudes or plans and less factual enquiries on savings. Attitudes, however, have always played a strategic role in these surveys and have been shown to be of significance when related to either saving or spending behaviour. Personal income expectations and general economic outlook, ascertained in re-interview surveys and in sequences of independent annual surveys, have been found to be sigificantly associated with savings. Buying intentions and general appraisals of purchasing conditions by consumers have been found to be useful guides to spending decisions, especially for the purchase

* This relationship does not vanish if home repairs and decorations are excluded from durable consumption expenditures.

† The British surveys are more consistent in that they enter capital transfers as positive savings in the accounts of donors. The Surveys of Consumer Finances do not, but both surveys by their very nature are biased in that they include interviews only with recipients and not with donors of inheritances.

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of durables. In the Oxford Savings Surveys we have found much weaker relations, if any, between consumer attitudes and savings. A fund of experience with data on buying plans has not yet been adequately established in the British surveys.

Demographic variables like age and family size are of obvious importance in the study of individual behaviour and have been duly examined in both the British and American surveys. Other less obvious demographic variables are race, city size, and region. In Britain there are no sharp contrasts to compare with the Negro-White differentials in America. Racial differences in savings behaviour which are, in turn, related to more fundamental socio-economic factors are not to be overlooked in the analysis of America's propensity to save, but there is no corresponding element in the U.K. on a scale of practical importance.

The size of city, in which survey respondents dwell, is, in the U.S., closely tied up with farm and non-farm occupational differences. It is felt by many students that the comparatively high propensity to save among farmers is not due solely to the entrepreneurial nature of their occupation, but partly, at least, to the peculiar nature of the rural way of life. Rural England is quite different economically and socially from rural America. We have found it useful to include farmers with other self-employed in the estimation of the British savings propensities but to distinguish between farm and non-farm self-employed in the estimation of the American saving propensities. Although the major distinction comes in America between rural and urban areas, which is nearly the same as that between farmers and others, Morgan has found some effects of city size on saving within urban areas alone. Some regional differences in American savings have been found between Northern and Southern Negroes. Other regional differences are largely explained by regional differences in income or other variables used in our equations. It is felt that the regional differences that do exist in Great Britain are similarly related to income or wealth differences.

Equations comparable to those estimated above from the Oxford Surveys have been estimated from three separate American samples—the re-interview sample of 1949 and the main survey samples of 1950 and 1951. In the re-interview sample, only 606 spending units are included; therefore all types of units are pooled together. In the main survey samples, there are more than 3,000 units; hence separate calculations were made for business owners, for farmers and for all other spending units. Generally speaking, these are representative samples of the entire population outside institutional quarters, but the re-interview sample covers only urban areas, i.e. cities and towns, of population 2,500 and over. In all instances, interviewing was done during the first quarter of the year and data refer to the preceding calendar year, the time of interview, or 12 months before the interview.

Re-interview Survey, 1949—

$$S/Y = \underset{}{-1{\cdot}36} + \underset{(0{\cdot}07)}{0{\cdot}41}\log Y/N - \underset{(0{\cdot}02)}{0{\cdot}23}\,L/Y + \underset{(0{\cdot}04)}{0{\cdot}06}\,\frac{Y - Y_{-1}}{Y_{-1}}$$

$$+ \underset{(0{\cdot}0017)}{0{\cdot}0038}\,A + \underset{(0{\cdot}05)}{0{\cdot}13}\,H$$

$$\frac{\partial S}{\partial Y} = 0{\cdot}38 \qquad R = 0{\cdot}52$$

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Survey of Consumer Finances, 1950—

Business owners:

$$S/Y = \underset{}{-1{\cdot}19} + \underset{(0{\cdot}07)}{0{\cdot}39}\log Y/N + \underset{(0{\cdot}37)}{0{\cdot}33}\log N - \underset{(0{\cdot}026)}{0{\cdot}064}\,L/Y + \underset{(0{\cdot}0000034)}{0{\cdot}0000038}\,L - \underset{(0{\cdot}00183)}{0{\cdot}00092}\,A$$

$$\frac{\partial S}{\partial Y} = 0{\cdot}33 \qquad R = 0{\cdot}36$$

Farmers:

$$S/Y = \underset{}{-1{\cdot}91} + \underset{(0{\cdot}13)}{0{\cdot}69}\log Y/N + \underset{(0{\cdot}23)}{0{\cdot}10}\log N - \underset{(0{\cdot}05)}{0{\cdot}33}\,L/Y + \underset{(0{\cdot}000022)}{0{\cdot}000055}\,L + \underset{(0{\cdot}0034)}{0{\cdot}0016}\,A$$

$$\frac{\partial S}{\partial Y} = 0{\cdot}47 \qquad R = 0{\cdot}52$$

Non-farm, non-business spending units:

$$S/Y = \underset{}{-0{\cdot}82} + \underset{(0{\cdot}03)}{0{\cdot}24}\log Y/N + \underset{(0{\cdot}04)}{0{\cdot}16}\log N - \underset{(0{\cdot}01)}{0{\cdot}19}\,L/Y + \underset{(0{\cdot}000003)}{0{\cdot}000029}\,L + \underset{(0{\cdot}0006)}{0{\cdot}0010}\,A + \underset{(0{\cdot}02)}{0{\cdot}05}\,H$$

$$\frac{\partial S}{\partial Y} = 0{\cdot}18 \qquad R = 0{\cdot}41$$

Survey of Consumer Finances, 1951—

Business owners:

$$S/Y = \underset{}{-2{\cdot}81} + \underset{(0{\cdot}13)}{0{\cdot}73}\log Y/N + \underset{(0{\cdot}22)}{0{\cdot}59}\log N - \underset{(0{\cdot}05)}{0{\cdot}13}\,L/Y + \underset{(0{\cdot}0000080)}{0{\cdot}0000047}\,L + \underset{(0{\cdot}0032)}{0{\cdot}0093}\,A$$

$$\frac{\partial S}{\partial Y} = 0{\cdot}56 \qquad R = 0{\cdot}41$$

Farmers:

$$S/Y = \underset{}{-0{\cdot}56} + \underset{(0{\cdot}10)}{0{\cdot}24}\log Y/N + \underset{(0{\cdot}18)}{0{\cdot}04}\log N - \underset{(0{\cdot}04)}{0{\cdot}37}\,L/Y + \underset{(0{\cdot}000014)}{0{\cdot}000068}\,L + \underset{(0{\cdot}00265)}{0{\cdot}00089}\,A$$

$$\frac{\partial S}{\partial Y} = 0{\cdot}37 \qquad R = 0{\cdot}47$$

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Non-farm, non-business spending units:

$$S/Y = -0{\cdot}79 + \underset{(0{\cdot}02)}{0{\cdot}25}\log Y/N + \underset{(0{\cdot}03)}{0{\cdot}10}\log N - \underset{(0{\cdot}01)}{0{\cdot}11}\,L/Y$$

$$+ \underset{(0{\cdot}0000018)}{0{\cdot}0000044}\,L + \underset{(0{\cdot}00047)}{0{\cdot}00057}\,A + \underset{(0{\cdot}014)}{0{\cdot}043}\,H$$

$$\frac{\partial S}{\partial Y} = 0{\cdot}18 \qquad R = 0{\cdot}36$$

These equations are not directly comparable with those computed for Great Britain, but they serve as a basis for the general remarks on similarities and differences made above. The American equations were not split between contractual and discretionary savings, and no windfall variable was included explicitly in the equations, although an analysis of residuals by classes of windfall receipts was made. An interaction is treated between liquid assets and income level and not between liquid assets and income change. In all but the re-interview equation income change is not used as a separate variable, and the number of persons in the unit is introduced in addition to its function as a deflator of income.

Apart from these differences the equations are similar. A useful statistic for comparison, one that avoids absolute monetary values, is the marginal propensity to save. In the 1950 and 1951 main surveys for the U.S., the marginal propensity to save for non-farm, non-business units is estimated as 0·18. For farmers and business men together it averages at about 0·40–0·45.

The estimated marginal coefficient for self-employed units in the 1953 Oxford Savings Survey is 0·43, on adding the results for discretionary and contractual savings. The 1954 estimate is only 0·30. The results for both years combined are below corresponding U.S. estimates, but the 1953 figure by itself is not comparatively small.

In the managerial and technical group from the 1953 Oxford Savings Survey, the result is decidedly lower (0·10) than for all non-farm, non-business units in the U.S. The managerial group is the second highest in the U.K.; therefore outside the self-employed group the British coefficient appears to be low. This result is confirmed by the estimates from the 1954 re-interview sample (Oxford data), in which the marginal propensity to save is estimated as 0·10 for all units and 0·07 for all employees.

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A Note on the Comparison of Exchange Rates and Purchasing Power Between Countries

By R. C. Geary

This note gives an extremely simple form of solution of a problem which has been much discussed in the literature and which is likely to assume increased importance in future, in connection, for example, with the institution of the European Free Trade Area and Common Market. It is well known that official exchange rates are not suitable as conversion factors for international comparisons in real terms of all economic flows of different kinds expressed in national currencies. The exchange rates between two countries may differ perceptibly for different flows, e.g. agricultural output, industrial materials, consumer purchases. Even if one favours an index of the Fisher type for comparison between two countries there is the difficulty that in general this index does not satisfy the "circular test", i.e. the computed exchange rates between countries A and B, and B and C, are not necessarily consistent with the computed rate between A and C. If one has to deal with n countries the number of pairs of comparisons is $n(n-1)/2$ from which one has to evolve n exchange rates (with one equal to unity), possibly by a system of averaging. This can be done in many ways, and the element of arbitrariness of choice is complicated further by the fact that one might feel bound to weight the individual indexes according to the importance of the countries involved.

Suppose that there are k commodities and n countries, the price and quantity of the ith commodity in the ath country being p_{ia} and q_{ia} respectively. Prices are expressed in the national currency whereas quantities are expressed in international units, quintals, number etc. The quantity unit may be different for different commodities. Each commodity is assumed to be identical in quality in the different countries.

Let C_i be the international price of commodity i expressed in terms of the international unit of currency, the "europa", and let the exchange rate for country a be E_a (= number of europas equivalent to a unit of the national currency). These entities are defined in reciprocal terms as follows:

$$C_i = \Sigma_a E_a p_{ia} q_{ia} / \Sigma_a q_{ia},\ i = 1, 2, \ldots, k \quad . \quad . \quad . \quad (1)$$

$$E_a = \Sigma_i C_i q_{ia} / \Sigma_i p_{ia} q_{ia},\ a = 1, 2, \ldots, n \quad . \quad . \quad . \quad (2)$$

This is a system of $(k + n)$ linear homogeneous equations in the $(k + n)$ variables C_i and E_a. One of the equations is, however, redundant. We find, in fact, from (1)

$$\Sigma_i \Sigma_a E_a p_{ia} q_{ia} \equiv \Sigma_i \Sigma_a C_i q_{ia}, \quad . \quad . \quad . \quad . \quad (3)$$

which can also be deduced from (2). Accordingly, both the C_i and E_a can be determined (proportionately) from the $(k + n - 1)$ in general independent equations in the system (1), (2). For practical solution the system can obviously be reduced to one of k or n (whichever is the smaller) homogeneous equations by substitution in (2) for C_i given by

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(1) or for E_a in (1) given by (2). The solution is particularly simple in the case of two countries when we find

$$\frac{E_1}{E_2} = \frac{\Sigma_i p_{i2}\left(\frac{2q_{i1}\,q_{i2}}{q_{i1}+q_{i2}}\right)}{\Sigma_i p_{i1}\left(\frac{2q_{i1}\,q_{i2}}{q_{i1}+q_{i2}}\right)} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

While this formula is "suitable" in form in that it is a price index with fixed weights, these weights turn out to be the harmonic means of the quantities! However, when $q_{i2} = cq_{i1}$, where c is a constant for all i, the ratio becomes Laspeyres in form, as one feels it should.* More generally if $q_{i2} = c_i q_{i1}$, where the c_i are large but vary with i,

$$\frac{E_1}{E_2} \sim \frac{\Sigma_i p_{i2}\,q_{i1}}{\Sigma_i p_{i1}\,q_{i1}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

so that the ratio of the rates of exchange would virtually depend on the quantity weights of the "smaller" country. On general grounds this is a less bizarre result than might at first appear.

In an interesting paper J. van Yzeren (1956) approaches this problem from several different angles, involving a consideration of weighted as well as unweighted formulae. Generally his unweighted formulae for the ratio of exchange rates in the two-country case are Fisher in form. He applies his methods to a highly artificial two-commodity four-country example as follows:

	Commodity 1		*Commodity* 2	
Country (a)	p_{1a}	q_{1a}	p_{2a}	q_{2a}
1 .	30	10	30	2
2 .	20	6	10	8
3 .	25	6	15	8
4 .	30	4	10	10

Van Yzeren's results applied to this example compare as follows with those using the method of this note (with $E_1 = 1$):

Values of E_a

		Van Yzeren					
Country	*Present Method*	I(a)	I(b)	II(a)	II(b)	III(a)	III(b)
1 .	1	1	1	1	1	1	1
2 .	1·66	2·02	1·80	1·72	1·92	1·87	1·86
3 .	1·23	1·52	1·38	1·27	1·42	1·39	1·40
4 .	1·37	1·53	1·28	1·44	1·69	1·48	1·47

(a) Unweighted; (b) Weighted.

In almost all cases van Yzeren's rates exceed those of the present method which, however, differ only slightly from those of his Method II(a).

The special feature which informs equations (1) and (2) is that, if the entities C_i and E_a exist, they could scarcely be defined reasonably in any other terms. In particular no problem

* This result is general, as (2) shows: any set of the n countries with proportionate q_{ia}'s will have the reciprocals of their E_a's proportional to the Laspeyres sum-products.

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of alternative systems of weighting seems to arise. The essential property of the system is, of course, the identity (3) which seems to be the analogue of the circular test (and a circular test which is fulfilled) in index number theory. The curious form (4) of the exchange ratio in the two-country application is a reminder that index numbers of the better-known type (the Laspeyres, Paasche, Edgeworth, Fisher in particular) cannot find their justification in economic theory: they have merely suitable arithmetical properties generally designed so that an amalgam of commodities behaves as if they were one commodity; and all lack one or other property which one would regard as desirable on the single-commodity analogue. If one derives exchange rates by index numbers or by any method other than the present one, the results will not, in general, satisfy the relations (1) to (3).

This theory was originally evolved to deal with the problem of computing a world index of agricultural output which does not involve implicitly the repudiation of the indexes for individual countries. If q_{ia} be the current annual output of commodity i in country a then, using the Laspeyres formula, Q_a, the volume index for country a, is given by

$$Q_a = V'_a/V_a \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

where

$$V'_a = \Sigma_i p_{ia} q'_{ia}, \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$V_a = \Sigma_i p_{ia} q_{ia}, \quad . \quad . \quad . \quad . \quad . \quad (8)$$

the unprimed symbols relating to the base period. Then the world index is

$$Q = \Sigma_a E_a V'_a/\Sigma_a E_a V_a, \quad . \quad . \quad . \quad . \quad . \quad (9)$$

where E_a is the agricultural exchange rate for country a in the base period.

Reference

VAN YZEREN, J. (1956), "Three methods of comparing the purchasing power of currencies". *The Netherlands Central Bureau of Statistics, Statistical Studies, No. 7.*

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Generalization of Yastremsky's Formula for Analytical Graduation of Fertility Rates

By Ivo Lah

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The Russian statistician Yastremsky (Bojarski, 1945, p. 245) established the following formula for the analytical graduation of empirical specific age rates of fertility

$$f(x) = f(m)\left(1 + \frac{x-m}{m-\alpha}\right)\left(1 - \frac{x-m}{\omega-m}\right)^{\frac{\omega-m}{m-\alpha}} \quad . \quad . \quad . \quad (1)$$

in which:

α = lowest age of fertile women,
ω = highest age of fertile women,
m = age of maximum fertility,
$f(m)$ = maximum annual fertility.

Yastremsky asserts that this formula (1) better corresponds to facts than the well-known Gompertz-Makeham's formula for analytical graduation of mortality.

Gompertz and Makeham illustrated the phenomenon of death with the "force of mortality" which increases in the geometrical progression with the age of the persons. The force of mortality is to be measured by its effect or intensity $\mu(x)$ i.e. with the change of the number of survivors lx in the infinitesimal time dx computed on the unit of the function lx and on the unit of time:

$$\mu(x) = \frac{d\,lx}{lx\,dx} = A + BC^x \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The integral of (2) is

$$lx = KS^x g^{c^x} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Like Gompertz-Makeham's idea we shall illustrate the phenomenon of birth with the "force of fertility" which is to be measured by intensity of fertility

$$\mu(x) = \frac{df(x)}{f(x)\,dx} \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

In the lowest fertile age α is $f(\alpha) = 0$, whereas the increment $df(\alpha)$ is a positive value. Therefore

$$\mu(\alpha) = +\infty \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

In the age of maximum fertility m is $f(m)$ a positive value, and $df(m) = 0$. There-

$$\mu(m) = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

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In the highest fertile age ω is $f(\omega) = 0$, whereas $df(\omega)$ is a negative value. Therefore

$$\mu(\omega) = -\infty \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

The fertility of various groups of women being different, the intensity function (4) has to contain at least one parameter which characterizes the typical properties of the fertility of observed women. Let us first suppose that there is only one parameter

$$a = \text{constant} \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

The simplest analytical function that meets conditions (5) to (8) is

$$\frac{df(x)}{f(x)\,dx} = a\left(\frac{1}{x-\alpha} - \frac{\omega-m}{m-\alpha}\frac{1}{\omega-x}\right) \quad . \quad . \quad . \quad . \quad (9)$$

The integral of (9) is

$$f(x) = ce^{a\left[\ln(x-\alpha) + \frac{\omega-m}{m-\alpha}\ln(\omega-x)\right]} \quad . \quad . \quad . \quad . \quad . \quad (10)$$

The constant c is to be determined by boundary condition $x = m$. Thus we get

$$\frac{f(x)}{f(m)} = \Phi(x) = e^{a\left[\ln\frac{x-\alpha}{m-\alpha} + \frac{\omega-m}{m-\alpha}\ln\frac{\omega-x}{\omega-m}\right]} \quad . \quad . \quad . \quad . \quad (11)$$

For the sake of space, let us set

$$\left.\begin{aligned} \eta &= \ln\frac{x-\alpha}{m-\alpha} + \frac{\omega-m}{m-\alpha}\ln\frac{\omega-x}{\omega-m} \\ \eta' &= \frac{d\eta}{dx} = \frac{1}{x-\alpha} - \frac{\omega-m}{m-\alpha}\frac{1}{\omega-x} \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (12)$$

Thus, we may write simply the differential equation (9)

$$\frac{df}{fdx} = a\eta' \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

and (11)

$$\Phi = e^{a\eta} \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

In the special case $a = 1$, (14) passes over to Yastremsky's formula (1), which can be still further generalized.

In practice one parameter only (8) is not always sufficient to show with enough precision all the typical characteristics of the fertility of observed women. For this reason several parameters $a_1, a_2, \ldots a_n$ are necessary. In order to simplify the integration let us put in (13)

$$a = \sum_{\nu-1}^{n} \nu a_\nu \eta^{\nu-1} \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

We get thus

$$\frac{df}{fdx} = \sum_{\nu=1}^{n} \nu a_\nu \eta^{\nu-1}\eta' \quad . \quad . \quad . \quad . \quad . \quad (16)$$

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The integral of (16) is

$$\Phi_n = e^{\sum_{\nu=1}^{n} a_\nu \eta^\nu} \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

and this is the required generalized formula for the analytical graduation of fertility. In the special case $a_1 = 1$, $a_2 = a_3 = \ldots a_n = 0$ (17) passes over to Yastremsky's formula (1).

The parameters a_ν in (17) are to be calculated by the least squares method

$$\left.\begin{aligned} &\sum_{x=\alpha+1}^{\omega-1} p(\Phi_n - \Phi_0)^2 = \text{minimum} \\ &p = \frac{L}{\Phi_0(1-\Phi_0)} \end{aligned}\right\} \quad . \quad . \quad . \quad (18)$$

in which represents Φ_0 the empirical values i.e.

$$\Phi_0 = \frac{f_0(x)}{f(m)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

and $L = L_x$ the number of fertile women x years old.

In the most practical way the graduation is feasible with natural logarithms of the functions Φ_n and Φ_0. The Taylor series of $\ln \Phi_n$ is

$$\ln \Phi_n = \ln \Phi_0 + \frac{\Phi_n - \Phi_0}{\Phi_0} + R \quad . \quad . \quad . \quad . \quad (20)$$

Remainder R, being a small quantity, might be neglected. Therefore we can set

$$\Sigma (\Phi_n - \Phi_0)^2 = \Sigma \Phi_0^2 (\ln \Phi_n - \ln \Phi_0)^2 \quad . \quad . \quad . \quad (21)$$

From (18) and (21) follows

$$\left.\begin{aligned} &\sum_{x=\alpha+1}^{\omega-1} P(\ln \Phi_n - \ln \Phi_0)^2 = \text{minimum} \\ &P = \frac{L\Phi_0}{1-\Phi_0} \end{aligned}\right\} \quad . \quad . \quad . \quad (22)$$

Varying the parameters a_ν we get from (22) the following system of normal equations, which is necessary and sufficient for the determination of $a_1, a_2, \ldots a_n$.

$$\left.\begin{aligned} a_1[P\eta^2] &+ a_2[P\eta^3] + \ldots a_n[P\eta^{n+1}] = [P\eta \ln \Phi_0] \\ a_1[P\eta^3] &+ a_2[P\eta^4] + \ldots a_n[P\eta^{n+2}] = [P\eta^2 \ln \Phi_0] \\ &\text{- - - - -} \\ a_1[P\eta^{n+1}] &+ a_2[P\eta^{n+2}] + \ldots a_n[P\eta^{2n}] = [P\eta^n \ln \Phi_0] \end{aligned}\right\} \quad . \quad . \quad (23)$$

The more parameters we have the better the graduated $f_n(x)$ approaches the empirical $f_0(x)$.

The computation will be shortened if we carry out the graduation separately for the age interval (α, m) and (m, ω).

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The intensity function of the first age interval might be

$$\frac{df}{f\,dx} = a\left(\frac{1}{x-\alpha} - \frac{1}{m-\alpha}\right) \quad . \quad . \quad . \quad . \quad . \quad (24)$$

Thus we get

$$\left.\begin{aligned} f_n(x) &= f(m)\, e^{\Sigma a_\nu \eta^\nu} \\ \eta &= \frac{m-x}{m-\alpha} + \ln\frac{x-\alpha}{m-\alpha} \\ \alpha &\lessgtr x \lessgtr m \end{aligned}\right\} \quad . \quad . \quad . \quad (25)$$

The intensity function of the second age interval might be

$$\frac{df}{f\,dx} = a\left(\frac{1}{\omega-m} - \frac{1}{\omega-x}\right) \quad . \quad . \quad . \quad . \quad . \quad (26)$$

Thus we get

$$\left.\begin{aligned} f_n(x) &= f(m)\, e^{\Sigma a_\nu \eta^\nu} \\ \eta &= \frac{x-m}{\omega-m} + \ln\frac{\omega-x}{\omega-m} \\ m &\lessgtr x \lessgtr \omega \end{aligned}\right\} \quad . \quad . \quad . \quad (27)$$

We have numerically tested our theoretical expositions on the fertility of Slovenian women in the period 1948–52 (Blejec, 1956). Since the author does not quote the numbers of fertile women, we assumed that L_x = constant. We assumed that the limit ages of fertility are $\alpha = 15$ and $\omega = 50$. For the age interval (15, 26) we obtained the following functions:

$$\left.\begin{aligned} f_1(x) &= 0{\cdot}166\, e^{2{\cdot}627\eta} \\ f_2(x) &= 0{\cdot}166\, e^{2{\cdot}553\eta - 0{\cdot}140\eta^2} \\ \eta &= \frac{26-x}{11} + \ln\frac{x-15}{11} \\ 15 &\leqq x \leqq 26 \end{aligned}\right\} \quad . \quad . \quad . \quad (28)$$

and for the age interval (26, 50):

$$\left.\begin{aligned} f_1(x) &= 0{\cdot}166 e^{3{\cdot}508\eta} \\ f_2(x) &= 0{\cdot}166\, e^{3{\cdot}890\eta + 0{\cdot}693\eta^2} \\ \eta &= \frac{x-26}{24} + \ln\frac{50-x}{24} \\ 26 &\leqq x \leqq 50 \end{aligned}\right\} \quad . \quad . \quad . \quad (29)$$

The values f_1 and f_2, computed by formulae (28) and (29), are given in Table 1 together with the empirical values f_0.

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BLEJEC, M. (1956), *Fertility Tables of Slovenian Women*, 1948–52. Ljubljana (Slovenian text).
BOJARSKI. (1945), *Course on Demographic Statistics*. Moscow: Gosplanizdat. (Russian text).

TABLE 1

Crude and Analytically Graduated Fertility Rates of Slovenian Women 1948–52

x	f_0 (%)	f_1 (%)	f_2 (%)
16	0·4	0·3	0·3
17	1·3	1·6	1·5
18	3·1	3·7	3·7
19	6·2	6·2	6·2
20	9·1	8·8	8·9
21	11·6	11·1	11·2
22	13·4	13·2	13·2
23	14·7	14·7	14·8
24	15·9	15·8	15·8
25	16·3	16·4	16·4
26	16·6	16·6	16·6
27	16·5	16·5	16·5
28	16·3	16·4	16·4
29	15·3	16·1	16·1
30	15·1	15·7	15·6
31	14·4	15·2	15·0
32	13·2	14·5	14·4
33	12·5	13·8	13·5
34	11·7	12·9	12·6
35	11·2	11·9	11·5
36	10·3	10·8	10·4
37	9·3	9·6	9·2
38	8·3	8·4	8·0
39	7·5	7·2	6·8
40	6·4	6·0	5·7
41	5·1	4·8	4·5
42	4·0	3·6	3·5
43	2·8	2·6	2·6
44	1·8	1·8	1·8
45	1·2	1·1	1·2
46	0·6	0·6	0·8
47	0·3	0·2	0·4
48	0·1	0·1	0·2
49	0·1	0·0	0·1

We are in a position to compute also the values $f_3, f_4, f_5, \ldots$ but we have renounced it, since the differences $(f_2 - f_1)$ already are not significant.

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REVIEWS OF STATISTICAL AND ECONOMIC BOOKS

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1.—*The Analysis of Multiple Time Series.* By M. H. Quenouille. London, Griffin, 1957. 105 pp. $8\frac{1}{2}''$. 24*s*.

In this monograph, the first of a series edited by Professor M. G. Kendall, the author shows that multivariate extensions of autoregressive processes can often be handled theoretically by a canonical analysis. The set of multiple time series is linearly transformed to another set of canonical variables each of which is generated by a simple Markoff scheme, the parameter of which may be either one of a conjugate complex pair corresponding to an oscillatory component, or alternatively a real number, in which case it may be treated as a "stochastic trend" or a superposed disturbance. Each of the original variables is thus a linear combination of these trend and oscillatory components.

I prefer a more direct derivation as follows. The process is expressed in the linear form $\mathbf{x}_t = \mathbf{B}\mathbf{x}_{t-1} + \boldsymbol{\epsilon}_t$ where $\mathbf{x}_t$ and $\boldsymbol{\epsilon}_t$ are p-vectors of random variables and $\mathbf{B}$ is a $p \times p$ matrix of structural coefficients. The characteristic equation of the process is then $|\mathbf{B} - \lambda\mathbf{I}| = 0$, and if the roots $\lambda_1, \ldots, \lambda_p$, are all distinct, they correspond to set of characteristic vectors $\mathbf{T}$, such that $\mathbf{T}\,\mathbf{B}\,\mathbf{T}^{-1} = \boldsymbol{\lambda} = diag\{\lambda_1, \ldots, \lambda_p\}$. Then, if $\mathbf{y}_t = \mathbf{T}\mathbf{x}_t$ and $\boldsymbol{\delta}_t = \mathbf{T}\boldsymbol{\epsilon}_t$, we obtain, at once, $y_{jt} = \lambda_t\, y_{jt-1} + \delta_{jt}$ $(j = 1, \ldots, p)$, which is the canonical form of the process.

However, the y variables corresponding to a multiple root must, in the general case, be treated as forming a block of relationships, since $\mathbf{T}\,\mathbf{B}\,\mathbf{T}^{-1}$ is not of the diagonal form $\boldsymbol{\lambda}$, except in trivial cases. Such a block cannot be reduced to canonical components; examples are (i) the univariate autoregressive model $x_t + 2\rho x_{t-1} \times \rho^2 x_{t-2} = \epsilon_t$ and (ii) the bivariate model $x_{1,t} = \alpha x_{1,t-1} + \beta x_{2,t-1} + \epsilon_{1,t}$ and $x_{2,t} = \alpha x_{2,t-1} + \epsilon_{2t}$, each of which possesses a double characteristic root and no canonical form. The monograph makes no mention of the multiple-root problem, though a convenient method given for obtaining the multi-

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variate "matrix autocovariance generating function" based on the canonical roots is still valid in the multiple roots case if the resulting indeterminate forms are replaced by limits. The author then goes on to show that the covariance structure cannot be used to determine the generating model, the form of which must be specified *a priori* in sufficient detail if its determination is to be completed from a knowledge of the covariance functions. This process of determination is labelled "identification", the terminology thus diverging somewhat from usage in e.g., econometrics and psychological factor analysis, where the meaning is reversed, i.e. given the hypothetical form of the generating model, the identification problem poses the question, "can the structural parameters we are interested in be determined from available estimates of functions of these, and perhaps other, parameters?" It is here, I believe, that the main methodological differences in emphasis and approach between author and reviewer reveal themselves.

In subsequent chapters, the author deals with large sample significance tests and estimation theory. This approach is supported by the analysis of five sets of series of 100 terms each, each generated by a different set of simultaneous stochastic difference equations. Although an electronic digital computer was employed to lighten the burden of analysis, these experiments were not replicated and so, perhaps, lack any statistical validity except as "demonstrations". Large-sample partial-correlation tests based on the χ^2 distribution are given, and although the claim is made that these are satisfactory for series of 100 observations, no real support for this assertion is provided. Some misleading points are (i) insufficient warning against the instability of estimates of the form C_s/C_{s-1} and $\mathbf{C}_s\,\mathbf{C}^{-1}{}_{s-1}$ $(s > 1)$; (ii) the statement (6.7) that "it is clear that, for moving average schemes the $\mathbf{C}_s$ of lowest order provide a set of sufficient statistics. Indeed, since the $\mathbf{\Gamma}_s$ of higher order disappear there is no point in considering them". $\mathbf{\Gamma}_s$ and $\mathbf{C}_s$ are respectively the sth—order theoretical and sample covariance matrices, and in fact the $\mathbf{C}_s$ of *all* orders are required for a *fully efficient* estimate of any of the non-null $\mathbf{\Gamma}_s$, no sufficient statistics being available; (iii) the treatment of "end corrections" (5.6). (iv) The invalidity of the large sample theory of estimation and testing, where roots of modulus greater than unity appear, seems to be disregarded.

Although most econometricians will not be surprised that a bad fit is obtained in the author's "practical example" (which is a study of the U.S. hog cycle), and will probably feel that the selection of variables and their resulting canonical components are very arbitrary, they will find, nevertheless, much to interest them in this study.

J. WISE.

2.—*Probability and Scientific Inference.* By G. Spencer Brown. London, Longmans Green, 1957. ix, 154 pp. $7\frac{1}{2}''$. 15*s*.

The author of this little book thinks that he has found a serious defect in the theory of probability. His purpose is to analyse the meaning of words like "probable" and "random". The criticism is destructive; the synthesis of a new theory (for he recognizes that some theory is necessary) is left to a later date. Five very short chapters totalling 31 pages are devoted to philosophical questions: for example, four pages on causation. There follow two chapters on probability, largely from an "equally likely" viewpoint. A more substantial eighth chapter gets to the heart of Mr. Brown's difficulties. He there discusses two types of randomness: "primary" randomness which refers to the unpredictability of the next term in a series, and "secondary" randomness which refers to the non-existence of patterns in a series. He remarks that a series can possess one type of randomness without the other. A ninth chapter introduces an experimental paradox concerning significance levels. A tenth chapter is devoted to proofs of a few theorems of which the following is typical: If a finite population of N members contains $\frac{1}{2}(N + \sqrt{N})$ individuals of type A, and $\frac{1}{2}(N - \sqrt{N})$ of type B then the mean square deviation from $\frac{1}{2}n$ of the number of individuals of type A in a random sample of size n from the population is the same as if the population contained equal numbers of elements of types A and B. (Special Theorem of Selections, p. 75). The mean square deviation is incorrectly called the variance ($\frac{1}{2}n$ is, of course, not the mean). The result is immediately deducible from standard results on

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sampling from a finite population. The next chapter discusses bias and stretch. Your reviewer had great difficulty in understanding many of the author's definitions and that of "stretch" completely defeated him: comment on this chapter is therefore impossible. Chapter eleven contains strong words about Bernoulli's theorem, which is described as "one of the most flagrant metaphysical excesses ever to pass as an adjunct to modern science". Three final chapters deal with practical applications of the author's ideas, in particular to chance machines. There are two appendices, the second of which is long (23 pages) and provides practical examples of non-randomness in ostensibly random series, due to the author and to a critic, Mr. A. T. Oram. A commentary, presumably written at the proof stage, concludes the book.

Most of Mr. Brown's troubles spring from the fact that he will speak about absolute probabilities and ignore the fact that all probabilities are conditional. Thus he refers to the probability of an event—full stop—and not to the probability conditional on some other event. We can often use apparently absolute probabilities throughout an argument only because the conditioning circumstances are fixed, but a finer analysis will reveal them to exist. The realization of this fact enables the severe criticism of Bernoulli's theorem to be exposed as fallacious. It also disposes of such trivial paradoxes as that of the fire practice (p. 62). The two types of randomness also yield to this analysis. Others of his troubles spring from the use of tail-area significance tests and are resolved by an honest use of Bayes' theorem. Aside from certain difficulties in comprehension mentioned above, in connection with "stretch", Mr. Brown's fears seem unfounded. But a reasonable extrapolation from the understood to the obscure would suggest that they too are imaginary.

The argument with Mr. Oram in the second appendix is interesting. Mr. Brown's argument is vitiated by the fact that he does not tell us how long he had to search before he found anything odd in Mr. Oram's results. If one looks long enough almost any series has peculiarities. Another trick is to test the range of n values as if it was the difference of two values taken at random. This pushes a significance level of 5 per cent. up to 29 per cent. ($n=5$) and 64 per cent. ($n = 10$). Mr. Brown's criticism of Soal's work has already been adequately dealt with by Soal and Bateman (*Modern Experiments in Telepathy*. London: Faber and Faber, 1954). D. V. LINDLEY.

3.—*Proceedings of the Third Berkeley Symposium on Mathematical Statistics and Probability*. Vol. I: The Theory of Statistics, ix, 208 pp. 45*s*. Vol. II: Probability Theory, viii, 246 pp. 49*s*. Vol. III: Astronomy and Physics, viii, 252 pp. 47*s*. Vol. IV: Biology and Problems of Health, viii, 179 pp. 43*s*. 6*d*. Vol. V: Econometrics, Industrial Research and Psychometry, viii, 184 pp. 43*s*. 6*d*. Berkeley and Los Angeles, University of California Press, 1956. London, Cambridge University Press. $10\frac{1}{4}''$.

This set of five substantial volumes constitutes impressive evidence, both of the success of the series of symposia organized by Professor Neyman and of the wide field of application now claimed for statistical method. Also, though possibly unintentionally, these volumes emphasize the relatively subsidiary role played by statistics in the general picture of scientific research. Accepting the essentially ancillary nature of statistical method, it is, however, encouraging to see the great variety of lines of work in which it may prove useful.

Of the present set of five volumes the first two are, perhaps, of most interest to the majority of mathematical statisticians, while the fifth contains a number of papers which should be of interest to the applied, and particularly the industrial, statistician.

The first volume contains sixteen papers of which eleven are concerned with general or special problems of estimation. There are two papers on non-parametric tests, two on decision theory, and one, of a general nature, on experimental design. A notable characteristic of this volume, as compared with earlier work, is the decline in the relative importance accorded to formal significance tests. For the applied statistician, J. Berkson's paper on various methods of estimation in the dosage-mortality situation should be of special interest. This paper investigates and compares the properties of a number of techniques and recommends for practical use a simple method which appears to be remarkably efficient. On the more theoretical side there are papers of particular interest by Chernoff and Rubin

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on the estimation of the location of a point of discontinuity in a probability density function and papers of a rather general nature by W. Hoeffding ("The role of assumptions in statistical decisions") and H. Robbins ("An empirical Bayes approach to statistics"). S. Ehrenfeld's paper on "Complete class theorems in experimental design" may be the starting point of a useful series of researches in this field.

The papers in the second volume are, as might be expected, of a rather more abstract nature than those in the first volume, and they require, in general, a higher degree of mathematical training for their appreciation. Of the fifteen papers in this volume, four are of a general, more or less fundamental, nature, four deal with problems in the theory of random functions, two are on the theory of Markov chains and processes, four are on applications of probability to particular problems in statistics, physics and engineering and there is one paper ("Ranking limit problem" by M. Loève) in which a number of interesting results on the limiting joint distributions of sets of ranked variables are obtained. Even the four papers on special applications of probability are couched in general terms, the main interest of the authors evidently lying in the probability theory rather than in the applications. The reviewer found a paper by E. Lukacs ("Characterization of populations by properties of suitable statistics"), on conditions under which properties of linear and quadratic forms define the population distribution, especially interesting among the papers in this group. Of the four papers on random functions, J. Hammersley's paper "The zeros of a random polynomial" is particularly well worth reading. An interesting paper by J. Kampé de Feriet on "Random solutions of partial differential equations", which appears in the third volume of the *Proceedings*, might well have been included in the second volume. Among the papers on the more fundamental aspects of probability theory there may be mentioned D. Blackwell's work ("On a class of probability spaces") in which certain anomalies in the standard theory based on measure on a Borel field of subsets are eliminated by a reasonable modification of the definition of the field.

Jumping now to the fifth volume we find four papers on econometrics, three on industrial research, and three on psychometry. Of the four papers on econometrics, K. J. Arrow and L. Hurwicz's paper "Reduction of constrained maxima to saddle-point problems" is essentially on a problem in pure mathematics, while E. W. Barankin's "Toward an objectivistic theory of probability" could quite well have appeared in volume two, or perhaps in volume one.

The two remaining papers in this group are general and philosophical, rather than practical, in approach. The three papers on industrial research, on the other hand, contain detailed factual considerations of their subject-matter. "Continuous sampling plans", by A. H. Bowker, contains little fresh material, but is a comprehensive survey of developments in this field in the past fifteen years; "Fractional replication in industrial research", by C. Daniel, gives new methods of deriving fractional replicate designs for 2^n experiments and a discussion of their practical use under varying circumstances, including possible application in sequential experimentation; M. Sobel ("Sequential procedures for selecting the best exponential population") discusses the problem of selecting the population with greatest expected value (for a particular character) out of k given populations—a problem which he, together with his collaborators, has discussed, for Normal populations, in recent issues of *Biometrika*. Of the three papers on psychometry, F. Mosteller's "Stochastic learning models" seemed, in the reviewer's opinion, to be of most interest, though this judgment may well be biased by the relative novelty of the contents of this paper.

No attempt is made in this review to give any detailed discussion of the contents of volumes three and four of the *Proceedings*. The papers in volume three are mostly concerned with colour luminosity relationships for stars and with the clustering of galaxies; among the papers in volume four there are three on epidemiology which are mainly mathematical in character. One of these is a brief paper by A. T. Bharuche-Keid; the others, by the British authors M. S. Bartlett ("Deterministic and stochastic models for recurrent epidemics") and D. G. Kendall ("Deterministic and stochastic epidemics in closed populations") contain substantial contributions to the theory of epidemics, together with numerical examples to illustrate the theory.

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The five volumes of the *Proceedings* contain work which has formed the basis of much further research in the intervening two years and undoubtedly will continue to do so. They also contain some material which is probably rapidly being forgotten, even by the authors. But, on balance, they constitute an outstanding contribution to the advancement of statistical theory and practice and will remain for many years an important source of reference.

N. L. JOHNSON.

4.—*Introduction to Modern Statistics.* By Werner Z. Hirsch. New York and London, Macmillan Co., 1957. xiv, 429 pp. $9\frac{1}{4}''$. 45*s.* 6*d.*

This is another American text-book written primarily for students of economics and business who take a course in statistical methods. The author therefore begins at the beginning with arrangement and presentation of data in frequency distributions, measures of average and dispersion. He goes on to deal with probability and sampling and estimation and tests of hypotheses for means and percentages. The use of the t distribution is explained. Chapters follow on index numbers, regression and correlation, time series, and the χ^2 test for contingency tables. Less usual is the inclusion of a chapter on quality control and here brief mention is made of the Poisson distribution. Only school arithmetic and algebra is assumed and no attempt is made to derive all the formulae mathematically.

The emphasis is "modern" in the attention given to statistical inference. The author chooses to introduce confidence intervals before statistical tests, and deals with means, totals and percentages. However, he may cause some confusion by initially using "confidence intervals" to describe limits placed on the value of a single item drawn from a fully known distribution. Confidence intervals are also given for the correlation coefficient and for a value estimated from a regression line. When statistical tests are introduced, possible alternative hypotheses are specified and the power of the tests examined. Testing is also followed through in later chapters and tests are given for trend and oscillation in time series.

The last chapter describes the operation of electronic computers and mentions the recent development of linear programming, input-output analysis and game theory. While this should make stimulating reading for the student it seems out of place to discuss electronic computers without even mentioning the more humble aids to tabulation and computation which are much more likely to be available to him.

The distinctive feature of the book is its style. The author introduces concepts with an example showing the need for them. For instance, the early chapters deal with the difficulties of the sales representative, Mr. C. C. Camel, of the "Lucie Tastes Better" Tobacco Company, who has to prepare for a conference and, because he is new to the job, can only go by data on past sales. He studies the data with the aid of a text-book by "Prof. Ekks". Many illustrations are given of the use of statistical methods in business and elsewhere and the exercises at the end of each chapter are framed in terms of practical situations. Having discussed estimation and testing, the results are used to derive the formulae for determining sample size and examples of their application are worked out. The text is enlivened by quotations and cartoons. Such light reading is especially likely to appeal to those for whom statistics is a compulsory subsidiary.

Unfortunately the text is not always clear, and there are a few misprints and errors. Of the latter, the author defines s^2 as $\frac{\Sigma(x-m)^2}{n-1}$ and then estimates the standard error of the mean by $\frac{s}{\sqrt{n-1}}$ (page 123). From later use of the t distribution it seems that the definition of s^2 is incorrect, although on page 124 the author refers to the standard error of the mean as $\frac{\sigma}{\sqrt{n-1}}$ where σ is the population standard deviation. The t distribution is introduced because for small samples "the sampling distribution of the means is no longer closely represented by the normal distribution", not because of the approximation involved

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in using the sample standard deviation. From the introduction of the normal distribution (page 51) it seems to be the entire type of symmetrical monomodal distributions which is "closely described" by the normal distribution curve. Similarly the binomial distribution only "well describes" (page 151) the sampling distribution of proportions. Of the misprints, those likely to confuse are on page 119 where the second type of distribution should be *sample* and not *sampling*, and on page 277 where the first four lines refer to the *dependent* and not the *independent* variate. Formula 8.5 on page 43 is correct only if $N = \Sigma w$ and it is not clear that it is defined in this way. On the previous page the formulae are correct, but to instruct correction for the change of origin before that for the unit is likely to lead to errors in practice. No instructions are given for calculation of regression coefficients using coded data, and the student is likely to need more guidance for his calculations.

For many students, however, these deficiencies will surely be more than compensated by the liveliness and interest of the style of presentation. RITA J. MAURICE.

5.—*Mathematische Statistik*. By B. L. van der Waerden. Berlin (etc.), Springer, 1957. ix, 360 pp. 9½". D.M. 46.

Professor van der Waerden is a well-known Dutch mathematician who has lived for many years in Switzerland. A different approach to this subject, in which few books have so far been written in German, was therefore expected.

It was encouraging to read in the preface that the book was preceded by many years of experience of advising biologists, engineers and economists, etc., and that the numerical examples were to be real practical ones. A little uneasiness was experienced on reading the list of three books suggested for further study as these consist mainly of advanced mathematics. In fact, the author uses just enough of this for a fairly rigorous treatment for practising research statisticians or for somebody teaching them, and does so at a level and style comparable with although different from Kendall's in *Advanced Theory of Statistics*. For the readers who know Joos's *Theoretical Physics* it is very similar. The main feature is a very careful elucidation in words of every step in a mathematical derivation, and it is thorough yet concise and readable. Most English-speaking readers will probably find the mathematical level a little more advanced than is really necessary, and the statistical ideas not advanced enough; some quite common pitfalls and difficulties are left out and many of the numerical examples are "errors of the third kind"* (Kimball, 1957) and give the right answers to the wrong problems.

A recent reviewer in this *Journal*† (Bailey, 1957) says that teaching mathematical statistics to trained mathematicians is much more straightforward than instructing other intelligent research workers. This is probably true, except for the appalling problem of deciding what to leave out. Inevitably a reviewer will disagree with the author's choice, or, for that matter, with almost any other choice by any author.

The first few chapters are mainly mathematics and probability; tests for differences between probabilities, and confidence levels, are introduced very early in relation to the binomial. There is even an elegant construction for approximate limits using ruler and compass. The author was one of the first to work out a remedy for inconsistencies in assigning confidence limits in samples from discontinuous distributions, and discusses the problem here, but does not follow it up. The Poisson distribution is derived as a law of rare events and a limit of the binomial, but not as arising from independent point events occurring at random that need not themselves be "rare". The negative binomial distribution is not mentioned.

Probability generating functions are not defined at all, nor are moment generating functions. These are replaced by characteristic functions, which are often used, but cumulants are not mentioned. This makes it difficult to appreciate the central limit theorem, which is indeed proved asymptotically, whereas the statistician usually wants to know how the sum of a fairly small number of independent variates is distributed.

* A. W. Kimball, *J. Amer. Statist.*, **52**, 133 (1957).
† N. T. J. Bailey, *J. R. Statist. Soc.* A, **120**, 88 (1957).

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Multiple integrals and simultaneous equations in many dimensions, matrices, tensors, and quadratic forms are introduced but used very sparingly. For several important distributions often derived elsewhere by geometry in n dimensions the author succeeds by straightforward transformation of variables in multiple integrals. The χ^2 distribution is clearly derived as a square of a normal variate and is very thoroughly studied in many contexts. One unusual section justifies its use in contingency tables with even very small expected frequencies. Its close relationship to the Poisson distribution is not mentioned and the incomplete beta function is barely mentioned. In fact one would not learn from this book how many complete probability distributions are well tabulated: instead, the 5 per cent. and 1 per cent. points dominate. These are even used with great difficulty in an extreme value problem involving 31 sample variances of which 6 are finally eliminated as belonging to different populations, after astonishingly many F tests.

Fisher's method of estimation by maximum likelihood is fully explained, as is minimum χ^2, together with bias, efficiency, and asymptotic standard errors of the estimates. The examples of estimation are often unconvincing. One never wants an unbiased estimate for the probability of rejecting a batch in a single-sampling inspection scheme, whereas this could have been a nice example showing confidence limits in relation to Neyman-Pearson errors of the first and second kind. These are, indeed, carefully expounded. Fiducial distributions are not mentioned. More is needed on standard errors from Taylor-series.

Several methods of bio-assay are discussed. The Behrens-Karber method based on accumulated probabilities is preferred to probits or logits, but it is not pointed out that the latter are easily adaptable to unequal intervals of the variables corresponding to dosage. There is nothing about the very important problem of estimating a ratio with its confidence (fiducial) distribution. This is important too in linear regression, in which the usual standard errors are too formally given as matrix elements. One example of least squares fitting is based on the sun's passage through the zodiac: this required three pages of small type but is worth it. As another example of least squares regression an economic time series is fitted by a linear trend. Possible cycles are discussed, but here a good opportunity is missed of outlining autocorrelation as a remarkable means of simplifying many of them.

Correlation coefficients are introduced independently much later in the book. Their distribution in normal samples is considered at length, together with simplifying transformations and use of Student t. Partial correlation is included. Rank correlation is briefly treated, mainly for testing null hypotheses and not for estimation. Other ranking tests are considered in detail, rather as distribution free tests to replace ones based on normal distributions than for cases where it is reliable to rank and unreliable or impossible to measure. The sign test is advocated for its simplicity, but range as an estimate of standard deviation is not given. In support of Wilcoxon's test there is an unfair comparison with a Student t test applied to a very skew distribution, but it is not pointed out how useful normalizing transformations are, nor even how to transform distributions. In fact there is little interest in distributions as such, more in estimation, and far more in tests.

Probably this lack of interest is responsible for one (small) mathematical inaccuracy (the only one found) where in deriving Sheppard's corrections the existence of distributions other than bell-shaped ones is overlooked. (Rounding off is the only kind of observer error studied.) A bimodal distribution for 16,000 brown beans is shown and a χ^2 goodness of fit test of normality is uncritically applied to it although the deviations nearly form a regular pattern. It looks like a superposition of 2 or possibly 3 asymmetrical distributions which might be log-normal. It is mentioned without any criticism that this distribution was analysed (not by the author) into 11 Normal ones.

All this must not give the impression that the book is haphazardly arranged, for this review has been mainly a random walk not touching the boundaries. A genealogical tree shows how the chapters are related. There is a short English-German dictionary of statistical terms, and an index of fields of application of the numerical examples. The reader must look upon nearly all these very critically; and the statistical concepts are not nearly as good as the exposition of mathematical principles and derivations, which is usually excellent.

M. E. WISE.

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6 .—*Mathematics and Statistics for use in Pharmacy, Biology and Chemistry.* By L. Saunders and R. Fleming. London, The Pharmaceutical Press, 1957. x, 257 pp. $8\frac{3}{4}''$. 27*s.* 6*d.*

This book deals with the following subjects: arithmetic, including logarithms; algebra, including complex numbers and quadratic equations; graphs, and the equations of the circle and conic sections in their simplest forms; series; calculus; trigonometry; differential equations; probability; means, variances, standard errors, normal distributions, t; tests of significance, espicially t, χ^2, and simple analysis of variance; the use of statistics in biological assay and in pharmacy. The book is intended mostly for pharmaceutical students, and includes a number of references to the *British Pharmacopoeia.*

The book is written in a very concise style. The "explanations" are often purely formal, thus complex numbers are introduced as follows: "Quadratic equations in which $b^2 < 4ac$ occur in the theoretical treatment of a number of physical problems, and it is necessary therefore to be able to solve them. To do this a new quantity "i" is introduced. This is not a simple number, but has the property $i^2 = -1$. . . . Numbers that contain i are called complex numbers".

This is considered a sufficient explanation of i, and no mention is made of the Argand Diagram. The same kind of formal explanation occurs in the statistical part of the book, for example, on p. 157, "Regression gives the best-fitting relation between two quantities, one of which is liable to chance error". It certainly saves a great deal of time and trouble if the hard-pressed student is willing to accept this kind of explanation without asking awkward questions, and such a treatment may be sufficient to meet the needs of the pharmacist, who is not in any way a professional statistician or mathematician. But one cannot help wondering how much real understanding such a student will have.

It should be added that in other respects the explanations are almost always very clear and there are a number of shrewd comments. There are a few places where one could wish that the authors had been more careful in stressing possible pitfalls; e.g. the discussion of the use of probability graph paper on p. 148 gives the impression that it provides as accurate a method of finding a mean and standard deviation as the ordinary direct calculation. However, these are only minor blemishes. On the whole the authors have succeeded in compressing an enormous amount of useful information into a small space.

CEDRIC. A. B. SMITH.

7.—*The Fascination of Numbers.* By W. J. Reichmann. London, Methuen, 1957. 176 pp. 8″. 15*s.*

Although this book has little connection with statistics it may be worth mentioning here, for any statistician in his off-duty moments will find much fascination in exploring the simpler properties of prime numbers, series, recurring decimals, magic squares, and so forth. Incidentally, the method of multiplication shown on p. 73 looks cumbrous but in practice it is usually quicker and easier than the standard method at present taught in schools, especially if it is slightly rearranged as follows:

$$\begin{aligned} 318 \times 652 &= \begin{matrix} 06 \\ 1502 \\ 180516 \\ 0640 \\ 48 \end{matrix} \\ &= 207336 \end{aligned}$$

(Method: write the products $3 \times 6 = 18$, $3 \times 5 = 15$, $3 \times 2 = 06$, in a diagonal line going upwards to the right; then follow with $1 \times 6 = 06$, $1 \times 5 = 05$, $1 \times 2 = 02$ in a second diagonal below and to the right of the first, and so on: finally add to get the product. Although more figures are written down than in the standard method the additions are all done in a single operation at the end, saving time and energy in the long run.)

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8.—*Digital Computer Programming.* By D. D. McCracken. New York, Wiley, 1957. London, Chapman & Hall. vi, 253 pp. 9″. 62*s*.

This elementary introduction is intended for readers who have had no experience of electronic computers. It concentrates entirely on programming and, quite rightly in a book of this nature, makes no mention of electronic circuitry. To illustrate his examples the author devises a "mythical" computer which has most of the desirable properties of modern computers without many of their disadvantages. This tends to make the illustrative programmes simpler than would be the case with a real machine but it does result in a greater generality than would be possible if one particular real computer was used.

The contents fall naturally into two sections: Chapters 1 to 9 on fundamental techniques and Chapters 10 to 18, together with the appendices, on more advanced methods. The first nine chapters with the exception of Chapter 3 are written in a very lucid manner. Order Structure, Scaling Problems, Loops, B-modification, Flow Diagrams and Sub-routines are all clearly explained. Attention is given to difficulties which often cause beginners much trouble, e.g. cycling a loop exactly the required number of times and resetting orders at the start of sections in which they are altered.

Not all programmers will agree with the author's recommended notation for Flow Diagrams. He proposes a system based on squares, circles and several non-symmetrical figures. The usual system of rectangular boxes joined by arrowed lines seems equally efficient and less complicated. Chapter 3 is devoted to binary and octal number systems. Much of this seems out of place and readers will be well advised to skip most of it, as indeed is suggested in a footnote.

In comparison with the first section, the second part of the book is disappointing. It covers such topics as Floating Point Methods, Interpretive Routines, Double Precision Arithmetic, Automatic Coding etc., but to explain their use and purpose in elementary terms requires more space than is allocated to them. For example, Magnetic Tape Programming (Chapter 12) has eight pages of text. A newcomer to programming will gain from this chapter a vague idea of what magnetic tape involves but no more. In the reviewer's opinion the book would have been greatly improved by the addition of about 50 extra pages of supplementary text to later chapters—surely the book is expensive enough to permit an expansion to 300 pages or so?

Despite these criticisms, a mathematically minded reader who works conscientiously through the text, performing the exercises left for him at the end of each chapter, ought to acquire a sound knowledge of programming techniques and, after study of the relevant machine manual, should be able to make a reasonable attempt at programming simple problems of his own for whatever computer is available. S. LIPTON.

9.—*Regression Analysis of Production Costs and Factory Operations.* By Philip Lyle. 3rd ed. Revised by Leonard H. C. Tippett. Edinburgh and London, Oliver & Boyd, 1957. xvi, 204 pp. 8½″. 16*s*.

The first edition of this book appeared in 1944, and was rapidly followed by a second in 1946. In recent years, however, it has been out of print and with Philip Lyle's death in 1955 the revisions that he intended for a third edition never became known. Mr. Tippett has now taken on the job of producing a new edition and there has seemed to him "to be nothing for it but to reproduce the second edition, with errors—as far as they are known—corrected". The book has thus not become "Lyle and Tippett" in the sense in which Yule's famous book became "Yule and Kendall".

In addition to the correction of errors Mr. Tippett has given a few explanatory remarks (such as "The subscripts have different meanings from those in Ch. IV") where the novice might have a moment of difficulty.

Any major revision of the text of the book would be a most difficult task, since the style of construction and the methods of working are so individual. Philip Lyle's idiosyncracies are seen in such features as the use of odds in preference to probabilities, the supplying of nomograms at the back instead of tables, and the fact that two-thirds of the text completes

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the main subject matter and the remaining third consists of a series of appendices full of tit-bits.

The main subject matter of the book is the fitting of regression lines to observations of weekly output and total costs for the week, and the difficulties are present which always arise when the statistician has to take data that are available rather than to collect his information by experiment. In the experimental situation the variations in the "independent" variable are present because the experimenter decided to impose them and the well-known words "other things being equal" are usually justified. In the industrial situation other things never are equal; if there is a change in output from one week to another there must be some cause for this which will not be merely the will of an experimenter. To analyse the figures without taking into account these causes of variation might lead to fallacious conclusions—if we have, say, the same unusually low output in each of two different weeks can it be assumed that the costs of those two weeks are observations from the same frequency distribution if the underlying causes of the low outputs are different?

Further, are we entitled to extrapolate the regression line, to refer to the intercept on the "costs" axis as fixed costs and then to split these fixed costs into overhead costs and factory fixed costs? The justification, on page 25, that the constant in the regression equation does supply the fixed costs for the range of output of the observations although they might change for other ranges of output is not wholly convincing. Perhaps the fundamental question here is—what do we mean by "fixed"?

In his preface to the new edition Mr. Tippett mentions some of the difficulties to be faced, and it is to be hoped that he will feel justified in incorporating some new paragraphs in the text itself when a fourth edition is called for.

The diagrams are all placed together at the end of the book. This is done deliberately, to make reference to them easy, since several of them are referred to more than once in the text. In practice it does not help. I found it necessary in reading the book to hold my fingers in the pages and to be continually turning back and forth. It would make the book far more readable to insert the pictures into the text. Several of them are used only once and for these it would be a clear advantage. For those that are used more than once a reference to their page numbers when they are mentioned again would be just as good as having them at the back.

The nomograms that are given for significance testing of r and t have been differently bound in the new edition, and this is a retrograde step. In the second edition the pages folded in such a way that, when in use, the nomogram was entirely beyond the area of the open book and with only a dozen pages of the book beneath the page to which it was attached. It was therefore fairly easy to lay it flat for use. In the third edition half the nomogram is within the area of the book and half beyond, and the half within has nearly 200 pages beneath it. Nothing appears to have been gained by this, and a good deal has been lost. In a future edition it might be better to do away with folding pages altogether—if the nomograms were reduced to two-thirds of their present size they would lose little in legibility and would then fit neatly into the size of page of the book.

The "proof" (footnote on page 131) that 0/0 is indeterminate, is fallacious, and might well be replaced by something better.

I. D. HILL.

10.—*Annual Epidemiological and Vital Statistics*, 1954. Geneva, World Health Organization, 1957. 617 pp. 11″. 50*s*.

This is the seventh yearbook on epidemiology and vital statistics published by the World Health Organization. Part I of the volume deals with vital statistics and cause of death. The populations of the various countries are given by sex for ages under 1 year, 1–4, and then in quinquennial groups to age 85 and a final group of 85 and over. The birth rate, death rate and infant mortality and the annual rate of increase are shown for each country, and for the countries for which data are available the neonatal mortality has been found. The birth rate in Europe in 1954 ranged from 14·6 per 1,000 in Sweden to 29·1 in Poland. The death rate varied from 6·9 per 1,000 in Iceland to 14·8 in the Isle of Man. The infant mortality was lowest in the Isle of Man, 16 per 1,000, and highest in Yugoslavia, 102 per

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1,000. The rate of natural increase was less than 5 per 1,000 in Austria, Belgium, Denmark, England and Wales, Isle of Man, Monaco and the Channel Islands and the rate was over 15 per 1,000 in the Faroe Islands, Iceland, Malta and Gozo, Poland and Yugoslavia. The number of deaths and the death rates by age are given for 42 countries, compared with 35 in the previous annual report. The number of deaths are given by age and sex for 50 different causes and for five countries where the age distribution was unknown the total deaths by sex and cause are given. The death rates for the numerically important causes of death by age and sex have been found. The number of deaths of children under 5 years of age are given by age and sex for selected causes of death. The first year of life has been divided into six periods. The chief specific causes of death among infants were, generally, diseases peculiar to childhood, congenital malformations, pneumonia and accidents. In Europe diphtheria as a cause of death is disappearing. No deaths under age 5 were registered from this cause in 1954 in Denmark, Northern Ireland and Sweden. The largest number of deaths from diphtheria was 450 in Italy, and 177 in Portugal was the second largest number. Deaths from tuberculosis have been tabulated by age, sex and site, the number of sites have been reduced from 11 in the preceding reports to 7. Any comparison of the trend of the mortality between various countries is affected by the accuracy of diagnosis; in some countries a death need not be certified by a medical practitioner. The main effect of this is from middle life onwards, and in 1954 the deaths due to senility or ill-defined causes varied from 1 to 20 per cent. of all deaths in the different countries.

Part II of the report is devoted to notifiable communicable and parasitic diseases. The seasonal distribution is given for 23 diseases in either monthly or four weekly periods, and for 11 other diseases which do not show any seasonal variation only the annual number of cases and deaths are given. Care must be exercised in attempting any comparisons between countries of the incidence or fatality of infectious diseases. Notifications are often incomplete, relating only to part of the territory or to some particular category such as hospital patients. Notification is not always compulsory or is only compulsory during an epidemic. For these and other reasons no attempt can be made by many countries to establish a fatality rate and it sometimes happens that the number of deaths exceed the number of cases.

In part III of the report the numbers of health personnel (physicians, dentists, midwives, nurses, pharmacists, veterinarians) are given for the various countries. The types of hospital, the number of beds and the number of vaccinations done against specific diseases are tabulated. This section of the report is not a complete summary of the facilities available, in some countries the figures relate only to government hospitals. The number of hospitals in England and Wales and in Scotland is not given although the number of beds is stated and the medical personnel relate only to those engaged in the Health Service. The number of vaccinations against whooping cough is not given for any part of the United Kingdom.

Despite the imperfections, which might be expected in this mass of data collected from various sources, the report forms a useful guide to epidemiological conditions and the trend within various countries.

W. J. MARTIN.

11.—*Social Aspects of Prescribing*. By J. P. Martin. London, Heinemann, 1957. xii, 180 pp. $8\frac{1}{2}''$. 21*s*.

The committee inquiring into the cost of the National Health Service—the Guillebaud Committee—frequently found themselves hampered by the serious lack of factual information on the working of the service and emphasized the need for further research. In a pioneering and valuable study Mr. Martin has filled in part of the gap on medical prescribing. He traces the history of prescribing from 1900 and shows how the administrative control of the scheme has changed from a quasi-judicial character under the National Insurance Act to educational under the National Health Service Act.

Against this historical background, an analysis is given of the prescriptions issued in

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1951 in sixty-seven medium-sized County Boroughs. It is worth noting that no new material was collected: the analysis was confined to information already available from one source or another. Altogether fifty-seven variables covering prescriptions, doctors' practices, patients, social conditions, geographical location and morbidity (ill-health) were examined. The reason for the choice of variable, the various sources of information and the problems associated with the classification of the data are indicated with clarity and precision. In the statistical analysis use is made of the produce-moment correlation, the phi coefficient ($r\phi$), matrix of concomitance, factor analysis, multiple regression equations and quartiles. Among other findings, it is concluded that, on the whole, the frequency of prescription per patient was a much more important factor in cost than the average cost of each prescription. The frequency of prescribing appeared to depend on climatic conditions, morbidity and local "custom" or, to put it in another way, prescription frequency was a response to ill health, the nature of the response being determined by local attitudes and expectations as to the appropriate behaviour of doctor and patient when illness occurred. The bearing which these and other conclusions have on the administration and control of prescribing are discussed at length.

This book, which is well written and arranged, will be of prime importance to the administrator, social worker and sociologist while the methods employed will be of considerable interest to the statistician and social scientist. A. BARR.

12.—*Population Theories and the Economic Interpretation.* By Sydney H. Coontz. London, Routledge & Kegan Paul. 1957. viii, 200 pp. $8\frac{1}{2}''$. 25*s.* 0*d.*

The purpose of this work is to demonstrate that demography has failed because it has become divorced from economics; and "to return population theory to its natural habitat, the field of economics".

This book does not make easy reading. The author claims with some justification that there are inherent difficulties in any attempt to categorize population theories as of any other complex structures. "Anomalies and peculiar combinations frequently obtrude and embarrass the formal simplicity . . . On the one hand, they may be overemphasized to the point of frustration so that no generalization is possible; or, on the other hand such difficulties may be ignored or insufficiently appreciated so that oversimplification results". Certainly the book does not err in the direction of oversimplification and the reviewer must confess to moments of frustration. Sometimes it appears that, like a defending counsel who defeats an action by playing one witness off against another, the author finds it necessary to display a vast array of contradictions to prove the insufficiency of particular theories. But, to be fair, patience is rewarded and the final ordering of evidence is achieved with considerable skill.

The author divides population theories into three groups (i) biological, i.e. those which contend that the law regulating population growth is fundamentally the same as that affecting the growth of animals and plants, (ii) cultural theories, i.e. those which relate population changes to mental characteristics of humanity, (iii) economic theories, i.e. those which assert that economic relations exert a preponderant influence. Each group is treated comprehensively with a wealth of illustration drawn from historical material and abundant references to other work.

Both biological and cultural theories are rejected; the former because, dogmatic rather than interpretive, they do not account for the behaviour of man as distinct from other animals, the latter because they are superficial and contradictory, though occasionally plausible. For example, in relation to biological theories it can be shown that in human populations fertility may or may not vary inversely with density; and in relation to cultural theories and concepts there has been failure to substantiate distinct urban and rural patterns of fertility and moreover in such studies of urbanization, intra-urban or intra-rural differentials have been ignored.

The main substance of the book is the examination of economic theories. The author stands firmly by the economic interpretation of history. He is at pains to point out that

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acceptance of the Marxist approach to historical explanation does not imply acceptance of other tenets of the Marxian system and quotes Seligman for the statement that "there is really nothing in common between the economic interpretation of history and the doctrine of socialism except the accidental fact that the originator of both doctrines happened to be the same man. Socialism is a theory of what ought to be, historical materialism is the theory of what has been". Marx's view that "every special historic mode of production has its own special laws of population, historically valid within its limits alone" is developed in a lengthy exposition of the theory of surplus value.

The development of this thesis in the hands of Soviet demographers has reached the conclusion that population growth is a function of the demand for labour. Further that other things being equal, "growth in population tends to increase productive efficiency. It makes possible a greater division of labour, a larger labour force, a greater interchange of skills and knowledge".

The author accepts this but finds the theory insufficient in its exploration of the mechanics of the relationship between the demand for labour and its supply mainly as a result of neglecting qualitative changes in demand for labour which raised its supply-price and of failing to appreciate that a rise in the level of living of workers is consistent with fertility decline.

To overcome this deficiency Dr. Coontz first traces the evolution of the economic function of the family both for rich and poor. In the poor there was no economic stimulus for family limitation as operated for the rich when the family ceased to be a production unit and the objective became the preservation of property inheritance. The author then goes on to make a historical review of changes in the demand for labour and their impact on fertility patterns and reaches the conclusions that "the fundamental determinant of fertility patterns is demand for labour and not the democratization of knowledge of effective means of contraception" and that "A continued fertility decline is a function of the slowing down in the rate of economic progress".

The main criticism that may be levelled at the book is that it fails to apply the labour-supply theory sufficiently explicitly either to current fertility differentials between various groups, e.g., differences between foremen and skilled workers, or to current world problems of population pressure. Further, although the final prophetic paragraph forecasts that demand for labour will not always govern supply, the author does not examine the present tremendous technological advances that tend to disturb the relationship between economic progress and labour demand.

This is an arresting and stimulating document and should be welcomed equally by those who find it controversial as by those who find it convincing. B. BENJAMIN.

13.—*American Families*. By Paul C. Glick. New York, Wiley, 1957. London, Chapman and Hall. xiv, 240 pp. 9″. 48*s*.

This volume is one of the Census Monograph Series, now customarily produced after the publication of the tabulated data of each population census in the United States.

Let there be no doubt: in its field this book is admirable and indispensable; but its field is not necessarily what the uninitiated reader might expect. The title might suggest that the author, starting from some human aspect of family life in the field of sociology, sought out data (perhaps of a statistical nature, perhaps not) throwing light on this aspect of human activity, and developed a clear picture in depth on a relatively narrow front. If it were the record of such a study, a reader in this field might well read the volume straight through from cover to cover with profit and pleasure. In a sense, therefore, the title is misleading, since the contents are almost the opposite of the hypothetical picture drawn above.

It is clear that the author, himself competent in sociological and statistical fields and, in addition, having inside knowledge of the handling of census and other official population statistics in the United States, has set himself the task of servicing all sociological and demographic research workers who will ever need to draw on American statistics of families in the mid-twentieth century, no matter what aspect of the family is involved.

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It is unquestionable that this objective is both appropriate and well worth achieving, but to compress the results of such a study into a mere 240 pages implies that therein will be found a commentary on widely differing aspects of the family and, further, lack of space permits each aspect to be treated in no more than its bare essentials.

It will be useful to indicate some of the very real difficulties that an outsider wanting to use American official population statistics would encounter, difficulties which the author is well qualified to overcome, and to a large measure does overcome in his book.

First, one would be generally unaware of the statistics available, and where they are published. Apart from an (appropriately) vague appendix giving general information on the American sources of such data, every table and diagram in the text shows precisely the published source from which it is derived. Thus, if one took grave exception to every single analysis the author has thought fit to provide, they would nevertheless be invaluable as a ready access to an authoritative statement of the available sources of relevant data.

Secondly, the definitions of terms (primary family, sub-family, secondary family, primary individual, secondary individual, household, etc), whilst in theory available in the volumes including the numerical data, in practice are difficult to find and, what is more important, changes of definition from time to time are particularly difficult to disentangle. This class of difficulty has been overcome by the author by some references in the text, and a comprehensive and clear appendix.

Thirdly, any user of statistics, including the professional statistician, is at a disadvantage compared with the statistician with inside information on the nature of the statistics. The author freely intersperses his commentary with remarks giving facts or guesses as to the nature of his statistics—usually the form of words makes clear what is fact and what guessed.

Finally there is a need for non-statisticians to get interpretations of statistics—as it were translations from arithmetic to humanity. The author endeavours to provide such interpretations in profusion, but here some criticism of his achievement can be made, and will be made later.

In brief then this volume is far more a work of reference than a work to be read through. It is necessary to indicate the range of studies to which it is applicable. A list of chapter headings will serve both to do this and also to demonstrate the relative ease with which a reader may find the section to which he wishes to refer :

Chapter 1 (Family Living Arrangements, 20 pp.) and Chapter 2 (Household and Family Composition, 32 pp.) deal with household composition as this term is used in the 1951 census of Great Britain. Chapter 3 (The Life Cycle of the Family, 18 pp.) and Chapter 4 (Changes in Family Composition during the Life Cycle, 17 pp.) on demographic aspects and Chapter 5 (Social and Economic changes during the Family Life Cycle, 15 pp.) on social and economic aspects of the family life cycle. The civil relationship of individuals, and in addition their living arrangements (e.g. whether living alone, with spouse, parents, etc.) are included in Chapter 6 (First Marriages and Remarriages, 27 pp.), Chapter 7 (Marriage Rates and Number of Previous Marriages, 18 pp.) and Chapter 8 (Separation, Divorce and Widowhood, 16 pp.). Population projections and forecasts in the context of the grouping of the population for living, i.e. in connection for instance with housing requirements, are dealt with in Chapter 9 (Future Household Formation, 16 pp.) and Chapter 10 (Future Family Formation, 12 pp.). Chapter 11 is a 10-page summary. There is also a 5-page bibliography, an 8-page index and three appendices: "Sources, definitions and explanations", "Comparability and reliability of census family data" and "Technical note on household and family projections".

The underlying objective of producing a guide to a class of American population statistics is achieved and many examples could be given of difficulties which the users of the volume might have and which the author foresees and overcomes. These favourable comments must be stressed if the wrong impression is to be avoided, since on secondary points the volume can be criticized.

The range of the volume is so great that the reader's interest cannot be held in straight reading, and it seems likely that the author's interest flagged at times too. In fact, the author

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is a recognized expert in the fields in which he was writing, and to suggest that this volume was to him a mere chore would be very wide of the mark. Nevertheless, volumes such as this can hardly avoid degenerating in places. Tabulations have been produced before their precise objective has been formulated—if formulated, it will merely be in general terms—and the author of the commentary has to do his best to draw the real life picture implied by the figures. For instance, in the present volume there are too many analyses of husbands and wives and bridegrooms and brides in terms of median ages, at marriage or census. There is no need to convince a statistical audience of the danger of picturing a distribution as a group concentrated at the average value. At times, and very properly, the author suggests cross-tabulations, not available to him, which would be enlightening.

This uphill battle, whilst manfully maintained by the author for the overwhelming majority of his text, clearly blunted his critical powers at times. The danger of drawing conclusions without standardizing, a most important point in demography, is remembered by the author time and again, particularly with regard to standardizing for age, but forgotten in relation to remarriage rate (a combination of relatively high rates for the divorced and low rates for the widowed). An even more serious mistake is made (pp. 151 and 152) when the author is trying to draw conclusions from a table showing median ages of girls (*a*) at marriage and (*b*) at divorce, with distinction of education. In effect the author subtracts median (*a*) from (*b*) to infer duration of marriage at divorce and hence concludes that college girls divorce sooner after marriage than high school girls. It is not merely possible, but virtually certain that women divorcing represent, not a cross-section of all married women by age-at-marriage, but a group biased towards early marriage. The fallacy in the author's argument derives from this and it seems more reasonable to explain the numerical pattern he has observed as due to the fact that more high school girls marry in their teens, with a high divorce risk, than do college girls.

The fault lies more with the tabulations than with the author, who was as it were trapped, and the point exemplifies the danger of attempts by ingenuity to circumvent lack of analyses giving direct information by deduction from indirectly related tabulations. This indirect use of tabulations applies somewhat generally through the volume and is undoubtedly excessive, though it must be admitted that few statisticians can resist this temptation. In the present case, few as the fallacious arguments are, it means that any statistical user of the volume must check each conclusion himself—the author makes no secret of his data and it is usually easy to check him—and any reader unable to do so must seek a second opinion.

To end on an unfavourable criticism would be unjust to the author, and it would be preferable to refer again to the considerable insight he has shown in the difficulties encountered by users of statistics, and the exceptional talent he has shown in forestalling these difficulties and producing an indispensable guide to mid-twentieth century census and other statistics of American households, to use the English terminology.

N. H. Carrier.

14.—*The Analysis of Family Budgets*. By S. J. Prais and H. S. Houthakker. Cambridge University Press, 1955. xx, 372 pp. 9½″. 30*s*.

This is an ambitious work since it not only aims to provide a handbook for the family budget analyst but also contains the results of detailed analyses of the British working and middle class family budget surveys, carried out just before the war. It is not really the fault of the authors that their ambitions have not been fully realized. The two surveys were not designed for this kind of econometric analysis; but every stage in the design of a survey is of course dependent on the purposes for which the results are intended to be used. The main purpose of the Ministry of Labour's 1938 survey of working class household expenditure was to provide the weights for a new index of retail prices, although the organizers naturally took some account, in a general way, of the interests of social scientists; the middle class survey was conducted on parallel lines in order to provide comparable information about middle class households. Since then, partly with the help of the in-

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formation provided by these two surveys, advances have been made in the techniques of analysis to which the authors and their colleagues at the Cambridge Department of Applied Economics have made notable contributions. Advances were continuing while this book was being written; and, just after it was published, J. Aitchison and J. A. C. Brown suggested an entirely new form of Engel curve which has some attractive properties. In the circumstances, the authors have done remarkably well to get as near as they have done to the target which they set themselves.

The book begins with a short account of the authors' theoretical model. Some of the exposition here (e.g. the section on quality variations) is none too clear, but most of the same problems are considered more fully in later chapters describing the practical applications, where their implications are more easily understood.

The difficulties involved in collecting accurate and reliable information from private households are then briefly discussed. The main emphasis here is on the different kinds of bias which are likely to affect the results. Probably because the authors lack first-hand experience in the organization of family budget surveys, little is said about other important problems, such as problems of sampling, the design of schedules, the coding and classification of the recorded information, the effects of variations in response rates on average expenditure, and so on.

The authors then consider some of the mathematical problems involved in fitting Engel curves to the data. They show that Durbin and Watson's test for serial correlation provides a measure of non-linearity. It can, indeed, be used to test whether the residual differences between the observations and the values given by any form of regression line are curvilinear. The authors also give two less rigorous methods of testing for the presence or absence of non-linearity.

They also discuss the problem of heteroscedasticity, i.e. correlation of the variance of residuals with any of the "determining" variables. They show that the variance of expenditure on some items appears to be directly proportional to the square of total expenditure. Heteroscedasticity implies that the estimates of the regression coefficients are inefficient, not that they are biased. Bias in the variances can be corrected by means of Theil's formula, but the authors find that the corrections needed to their own results are very small. The problem of obtaining efficient estimators is more complicated. It can sometimes be overcome by appropriate transformation of the variables; otherwise, the solution is likely to involve the method of iteration. Some of the forms of Engel curve which the authors have selected for investigation avoid this difficulty. For the others, they did not use most efficient methods of estimation, presumably because of the extra computational work involved (although they had the services of an electronic machine).

In a useful chapter on computational methods, the authors give us the benefit of their experience in having most of the computational work of their analyses done on the electronic computer at the Cambridge Mathematical Laboratory.

The analyses of the results of the two pre-war surveys are undertaken in four separate stages. The effects of *income*—for which total expenditure is used as a substitute—are first investigated. At this stage it is assumed that the effects of household size are adequately taken care of by using figures of average expenditure per head, and that there is no correlation between household composition (relative numbers of males, females, children of different age and sex, occupations etc.) and income. The second stage is concerned with the effects of income on the *qualities* of the commodities bought, on similar assumptions about the effects of household size and composition. In the third stage, the effects of household *composition* are investigated with the help of empirical estimates of unit-consumer scales. An attempt is made, in the fourth stage, to take account of economies of scale which may be expected to accompany an increase in the *size* of the household.

The authors were obliged to use total expenditure as a "determining" variable because no information about incomes was collected in the working class survey. But they argue that, even when figures of income are available, it is better to use total expenditure (they allow an exception in the case of durable household goods). "The use of total expenditure as the determining variable in the Engel curve can be justified", they write, "on the assump-

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tion that while total expenditure may depend in a complicated way on income expectations and the like, the distribution of expenditures among the various commodities depends only on the level of total expenditure." They suggest, in effect, that the amount spent on any one item is determined in two distinct stages, total expenditure being determined first and then the proportion which the item absorbs of the total. As a description of what happens, this seems unrealistic; we are interested in any case in knowing what determines total expenditure, which is not an independent causal factor. The amount spent on a particular item is a function, rather, of net income (including past and expected future income) and the possibilities of borrowing money or getting an overcraft from the bank. Even with hire purchase it is hardly true to say that expenditure on anything out of the ordinary is usually balanced by reductions in expenditure in the same week on other items. There was no need, however, to try to justify what was unavoidable.

Five forms of equation are applied to expenditure on food:

$$\log v_i = \alpha + \beta \log v_0 \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\log v_i = \alpha - \beta/v_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$v_i = \alpha + \beta \log v_0 \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$v_i = \alpha + \beta v_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

$$v_i = \alpha - \beta/v_0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

v_i denoting expenditure per head on the particular item and v_0 total expenditure per head. The authors show, as has been shown previously, that at any given level of income expenditure on luxuries (necessities) is lower (higher) in larger than in smaller households. These differences can be very roughly eliminated by using figures of average expenditure per head, the assumption being that similar proportions of households of different composition are found at all income levels.

When applied to six broad groups of food (farinaceous, dairy, vegetables, fruit, fish, and meat), equations (2), (1) and (3), in that order, show the highest correlations and (4) shows the lowest. When put through a linearity test, (3) comes out best, (1) and (2) practically dead-heat for second place, and (5) is easily the worst. The evidence seems to conflict with the assumption of a constant elasticity of demand, which eliminates (1) even though the authors seem reluctant to discard it. As far as foods go, either (2) or (3) seem reasonably satisfactory although both forms show evidence of non-linearity.

Regression coefficients were calculated separately for the working class and middle class samples and for the two samples combined, for the six broad food groups using equation (1), and for six broad non-food groups (rent, fuel, clothing, durables, literary, and "vice") using equations (1) and (3). For all the food groups and for three or four of the non-food groups the differences between the regression coefficients for the two samples appear to be significant. Correlation coefficients for the two samples combined show (1) coming out better than (3) on the non-food groups. The separate correlation coefficients for each of the two samples would have provided a better basis for comparing the different equations, since there is little reason to expect the same equation to apply to both the working and the middle classes, even if a linear term for social class is included; but these do not appear to have been calculated. Equation (3) was applied to each of 58 food items and equation (1) to each of 60 non-food items. As an electronic computer was available, it seems a pity that only two forms of equation were applied to the six broad non-food groups; that only one equation was applied to each of the smaller groups; and that, despite the evidence of non-linearity, no other non-linear forms of equation were tried. The reasons given for not using polynomial regression are unconvincing.

The authors' most original contributions have been to the study of quality variations. As the working class survey gave information about both quantities and expenditure for most kinds of foodstuffs, they were able to compare the average prices paid at different levels of income. They show that there is a regular relationship between the average price

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paid and income per head, which is reasonably well described by a semi-logarithmic equation of form (3) above. The quality elasticity is, of course, generally smaller than the expenditure elasticity which is the sum of the quantity and the quality elasticities.

An examination of detailed figures from a Dutch survey leads the authors to the conclusion that, in the case of fruit, increases in income are accompanied by increases in the average price paid for individual varieties but not, as might be expected—and as shown by vegetables and cheese—by a shift towards the more expensive varieties. This rum result is probably due to the arbitrary classification which defines the different "varieties": thus plums, which are relatively cheap, and peaches, which are usually dear, are both in the one group "stoned fruit" (which also seems to include melons and grapes); strawberries, raspberries, loganberries and blackberries (and red and black currants?), though sold at very different prices, are all in the one group "berries"; and so on. Whether a rise in income is accompanied by shifts from the less expensive to the more expensive varieties within groups or between groups depends on where the dividing lines are drawn. It is difficult to see how the groups can even be arranged in order of expensiveness since different fruits are purchased in different units which are not directly comparable. As the data distinguish 44 varieties of fruit, there is, as the authors themselves remark, "ample scope here for further analysis".

In examining the effects of household composition, the authors make use of the method of unit-consumer (or equivalent-adult) scales. The main difficulty here is that different scales are appropriate to different commodities; and the equation for any one commodity must take into account not only the specific scale for that commodity but also the income scale for all commodities combined. The use of unit-consumer scales thus involves a non-linear form of equation which makes the calculations very complicated. On the assumption that all consumers have the same value on the income scale (it should not be difficult to improve on this assumption), specific scales were calculated for the six main food groups and for all food combined, using two forms of equation. The only standard errors shown, those of the coefficients for adult females, range between 5 and 13 per cent. for the different food groups; the standard errors of the coefficients for some of the other types of person (e.g. infants) appear to be somewhat larger. The authors' claim that the first decimal place of the coefficients is well determined and that some confidence can even be placed in the second decimal place seems unduly optimistic. Curiously enough, they derive satisfaction from the similarity between the income elasticities obtained from these equations and those obtained in the first stage of the analysis, where no account was taken of differences in household composition. Not only would the more refined method be expected to produce different results; but the elasticities are, in fact, a good deal lower than those previously obtained from the same form of equation. The authors have not, in short, succeeded in demonstrating the general validity of unit-consumer scales which they have calculated only for food, from pre-war working-class data. The underlying *rationale* of the unit-consumer scale suggests that it is more likely to apply to necessities than to luxuries; and the scale may itself be a function of income. It remains doubtful whether the expenditure on all commodities by all types of household at all levels of income can be adequately described in terms of a uniform set of unit-consumer scales. Yet the validity of the scale for any commodity depends on this assumption.

The next stage of the analysis is concerned with the effects of economies of scale resulting from an increase in the size of the household. For this purpose the authors apply an exponential factor to the number of persons in the household. As before, they distinguish between the specific effect of a single commodity and the income effect which is the weighted average of all the specific effects. The income effect is estimated from the equation of the quality index for all food at 13 per cent. The estimates of some of the specific effects are somewhat larger and most of them are probably significant, although standard errors are not given. This stage in the analysis is, however, regarded by the authors as no more than "a preliminary venture in a new direction". In a final section, the authors take a cursory glance at the effects of social class, occupation and region.

Each main stage of the analysis deals with a single problem, the other problems being

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shelved for the time being. The authors recognize that a simultaneous attack on the various problems would probably produce different results and they regard this as the most important development awaiting further research. While the lack of integration may seem to be the main weakness of the book, the authors nevertheless appear to have been too ambitious. The results of each stage, with the exception of the analysis of quality variations, leave important questions unanswered. But progress is bound to be slow in what is still a fairly new field of research. The outstanding merit of the book, for which all who work in this field will be grateful, is that it sets out the analytical problems and the formal relationships between them clearly and systematically.

J. L. NICHOLSON.

15.—*Regional Income.* Studies in Income and Wealth, Vol. 21. By the Conference on Research in Income and Wealth. Princeton, University Press, 1957. London, Oxford University Press. x, 408 pp. 9″. 63*s*.

Comparatively little has been done in this country in preparing income estimates for different regions. There are no official estimates of the incomes produced in England, Wales, Scotland or Northern Ireland similar to the ones prepared for the United Kindgom as a whole. But in recent years several estimates have been made by private investigators notably for Wales by Professor Nevin, for Scotland by Mr. A. D. Campbell and for Northern Ireland by Professor Isles and Mr. N. Cuthbert.

In a country as vast as the United States the need for regional income estimates is more obvious than it is in this country and for a good many years a considerable amount of work has been done in preparing and interpreting such estimates. Official estimates of state incomes have been published since 1939, unofficial estimates were first published twenty years earlier.

In June, 1955 "regional incomes within the United States" was made the principal subject of a meeting of the Conference of Research in Income and Wealth. This book brings together the papers prepared for the conference together with the comments of participants.

The ten papers can be divided into three groups. The first is concerned with some of the conceptual issues generally applicable to any study of inter-regional differences. The second gives accounts of recent empirical investigations into regional incomes; and the third group discusses certain problems encountered in the estimation of county incomes.

The first group of papers, by Hochwald, Perloff, Isard and Ullman and Klove, is probably the most interesting to the English reader; and perhaps the most important of these is the paper by Hochwald who discusses the various conceptual and statistical problems involved in making estimates of regional incomes. One problem is the distinction between the incomes produced in a region and the incomes received by residents of the region; another is that of dealing with the incomes of corporate enterprises and other institutions whose activities range over more than one region.

Perloff in his paper is concerned with some of the major problems involved in assessing economic progress in different regions. Isard's paper describes briefly the value of the regional approach in economic analysis. Ullman and Klove describe some of the regional areas available for statistical use and summarize some of the factors influencing their formation. They also describe the methods followed by an official committee in establishing a set of regions.

The second group of papers is by Hanna, Hurwitz and Stallings, and Mansfield. Hanna's paper is concerned with problems encountered in analysing and interpreting differences among personal income receipts in different states. Hurwitz and Stallings present estimates of *per capita* real incomes for different states and find *inter alia* that there is little difference in price trends among states and regions. The third paper in this group, the one by Mansfield, shows that incomes tend to rise with city size.

The third group of papers, by Thompson, Fulmer and Shrycock, is concerned with short-cut methods of estimating incomes for small areas, the estimation of agricultural income, and intercensal estimates of population by countries.

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This volume, like so many of the others in the series, should be a useful source of reference to the specialist or student interested in regional incomes; it brings together the experience gained by investigators in the United States and throws much light on some of the problems encountered.

L. S. BERMAN.

16.—*A Theory of the Consumption Function.* By Milton Friedman. Princeton, University Press, 1957. London, Oxford University Press. xvi, 243 pp. 9″. 38*s*.

Many of the difficulties in economic theory arise from the fact that economic influences so often take a considerable time to work out their full effects. The eventual results of any change may be entirely different from the initial results: long-term elasticities of demand and supply may greatly exceed the short-term elasticities: the importance of such complications are, for example, well recognized in the theory of international trade.

The same phenomena arise in connection with the propensity to consume, under which title Keynes discussed the relation between disposable income and expenditure on consumption goods. The full effects of an increase in the income of a "spending unit" on its rate of "consumption expenditure" will not always appear within a week or a year; a decade or even a lifetime may be required before they are fully worked out.

One way in which this phenomenon is reflected in statistics is in the apparent contrast between the evidence of family budget studies and short-term time-series studies on the one hand and of long-term time-series studies on the other hand, concerning the comparative rates of savings of rich and poor. The family budgets and the short time-series suggest that increases in real disposable income per head are associated with increases in the savings-ratio (i.e. the proportion saved out of income): whereas long-term time-series studies suggest that the savings ratio for a community remains approximately constant over the decades although its real income per head shows a rising trend, with fluctuations superimposed. One possible interpretation of this contrast is that whereas the long-term elasticity of consumption expenditure per head to income per head (both being measured at constant prices) is unity,* the elasticities over periods of a year or a trade-cycle are appreciably less than unity.

A number of theories on these lines are by now familiar, which make consumption expenditure depend not only on current income but also on income in the past or on something correlated more closely with that than with current income. These theories have the advantage not only of reconciling the two sets of empirical evidence described above, but also of being easy to build in to trade-cycle models which would previously function properly only "on the flat", so as to enable them to function also for an economy with a secular increase in real income per head.

Professor Friedman has contributed some new ideas on the same lines. He gives the name "permanent income" to the collective influence of a family unit's past, present and particularly of its prospective income on its consumption expenditure. Its permanent income is not precisely defined but is what it regards as its normal income: its current income is only an imperfect indicator of its permanent income, which indeed cannot be measured since it is but a belief of the planning-head of the unit. Professor Friedman contends that consumption expenditure will be a proportion K of permanent income and that K will be independent of the level of permanent income, although it may depend on such other factors as the rate of interest and the security of the financial prospects of the spending unit. He bases this contention on a theoretical argument to which we will return in a later paragraph.

Current income is assumed to differ from permanent income by a disturbance which is uncorrelated with permanent income: likewise, consumption expenditure is assumed to differ from the appropriate multiple of permanent income by a disturbance uncorrelated with permanent income: moreover the two disturbances are themselves assumed to be

* It might however be found that if disposable income per head and consumption expenditure per head were expressed in wage units (instead of in constant prices) quite a stable relation existed between them although the elasticity was less than unity even in the long period. It would be best to use weekly rather than hourly wage-rates for making this calculation.

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uncorrelated. This confronts us with the familiar difficulties of estimating the regression between two variates both of which are subject to (uncorrelated) disturbances: given the results of a cross-section budget study for current income and consumption expenditure, the slope of the regression of expenditure on permanent income must lie somewhere between the two fitted regression lines (obtained by minimizing sums of squares of discrepancies in the expenditure and income directions respectively) but more cannot be said without further assumptions or knowledge of the relative sizes of the two sets of disturbances. The further assumption put forward by Professor Friedman is that the true regression is one of proportionality so that the line goes through the origin and consumption is a proportion K of permanent income, and independent of it.

This assumption has the merit that it might be capable of disproof if it were wildly wrong: for if both the fitted regression lines cut the consumption axis above the origin, no line between them could pass through the origin. However, since the observed regression lines of expenditure fitted by least squares to current income do not cut the expenditure axis very far above the origin (in the sense that the marginal propensity to consume is not found to be very much less than the average propensity to consume) and since the correlation between current expenditure and current income although naturally high is not very high (over 0·9 say) it is quite possible that the theory would survive this test.

The theory has the further advantage that it can be tested against some other theories which are to some extent similar. A large part of this book consists of a series of most ingenious arguments that the U.S.A. data support the theory well and better than such theories as those of Dewsenberry and Modigliani. An article recently published by Malcolm Fisher* and using English data also gives some support to the Friedman theory as against the Modigliani theory.

Many readers will have little difficulty in accepting the view that consumption expenditure depends in prosperous communities on long-term prospects of income rather than on current income: they may even find plausible the view that the proportion K of consumption expenditure to permanent income is itself independent of permanent income: but they may find Professor Friedman's theoretical basis for these views less acceptable. This basis is rather sketchily developed in Chapters II and III of his book.

Professor Friedman avoids the introduction of a cardinal utility function by using indifference maps concerning consumption in distinct periods. He places very strong restrictions on the form of the surfaces involved in these maps. One of these imposes a condition of symmetry of the curves relating consumption in each of any two periods: this assumption amounts to ruling out the phenomenon that was once familiar to economists as "time preference". A second and stronger condition could be expressed in the form that where $f(x_1\ x_2\ \ldots\ x_k) = \lambda$ is the equation of the family of indifference surfaces relating to consumption expenditure in each of K periods, $f(x_1\ \ldots\ x_k)$ should be homogeneous in the K variables. This implies that if an individual's expected real income were increased in some one proportion μ, the same in each period, then he would arrange to distribute his expenditure proportionately exactly the same as before: i.e. he would multiply his expenditure by exactly μ in each period. He argues that equal proportionate increases in consumption in any two years will affect in opposite senses the ratio of the "urgencies of additional consumption" in the two years and adds "These two effects need not exactly offset one another; but there seems no *a priori* reason why the first should systematically or generally tend to exceed the second or conversely; the things being compared are of the same stuff, differing only in dating; it is hard to see any reason why this difference in dating should have an asymmetrical effect. There seems nothing unreasonable, therefore, in supposing that the two effects exactly offset one another, and this is surely the simplest hypothesis". (He allows the possibility that empirical evidence will necessitate the abandonment of the hypothesis in favour of a more elaborate one.) This argument is a weak foundation for a whole book.

This second hypothesis rules out the possibility that the urgency of additional consump-

* *Institute of Statistics Bulletin*, August, 1956.

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tion becomes compelling when the level falls towards the minimum necessary for subsistence. Translated into terms of cardinal utility it is equivalent to assuming that the marginal utility of consumption at the level x is of the form $Cx^{-\alpha}$ with $\alpha > 0$. There are some *a priori* grounds for regarding some form such as $C(x - x_o)^{-\alpha}$ (when x_o is the subsistence level) as more truly reflecting behaviour—for this form suggests that a man will always, if possible, dissave rather than starve.

From these assumptions about indifference curves it can be deduced that in the absence of uncertainty and at a given rate of interest and for a given individual, consumption will be a constant proportion of "permanent income" where this latter is defined as that permanent constant flow of income which would have the same present discounted value as his actually expected (possibly variable) flow of income. Professor Friedman adds that this proportion "obviously does depend upon the interest rate": in the rarefied atmosphere where time-preference is ruled out this is far from obvious to your reviewer: suppose the indifference curves to constitute the family $\sum_{i=1}^{K} x_i^{-\beta} = \lambda$ where K is effectively infinite and x_i denotes consumption in period i then the proportion saved out of permanent income can be shown to be $\frac{1}{1+\beta}$ which is independent of the rate of interest.

If there were really no time preference the indifference surfaces would have to be of a very peculiar shape to justify only 12·5 per cent being saved out of permanent income: they would correspond with a state of affairs where the marginal utility of consumption varied inversely with the eighth power of consumption: at constant prices if one's income were £1,000, an extra £ of expenditure would have to be of less benefit than an extra penny of expenditure when one's expenditure was £500.* To the extent that this conclusion is silly, the fact that only one-eighth of income is saved contradicts the assumption that the indifference surfaces are symmetrical as between periods so that time-preference is absent.

However the flimsiness of the theoretical structure underlying the concept of permanent income and the constancy of the savings-ratio out of permanent income does not destroy the value of these concepts: a sounder rationalization of them will no doubt one day be provided for there is some sense in them: meanwhile they will provide a fresh viewpoint from which to survey empirical data. Just as Keynes's conclusions from the excess of the average over the marginal propensity to consume would still hold in the short period with which he was mainly concerned, even if the *a priori* part of his argument was wrong and Professor Friedman were right, so many of Professor Friedman's remarks about the empirical evidence relating to the consumption function may be sound even if his theoretical structure is not.

D. G. CHAMPERNOWNE.

17.—*The Investigation of National Income in British Tropical Dependencies.* By A. R. Prest. London, Athlone Press, for the Institute of Commonwealth Studies, 1957. 32 pp. $8\frac{1}{2}''$. 4*s.* 6*d.*

This is a slim booklet and the sections strictly relevant to the title are no more than half the contents. The first two sections are admirable summaries, in general terms, of the concepts of national income accounting and the use of the results in "Western" countries.

The success of national income studies in the dependent territories linked with the Colonial Office has been due to a fruitful partnership between official statisticians and university research workers, the most notable amongst the latter in recent years being Dr. Prest himself, Mr. Dudley Seers and Professor Peacock. There has been a healthy difference of views some of which have been debated in articles published in the *Review of Economic Studies*. In his booklet Dr. Prest has avoided most controversial topics and has only hinted at the concept which is one of the challenging features of his work (with Mr. Stewart)

* "Uncertainty" might well be used as an explanation in place of "time-preference": but Professor Friedman regards uncertainty as a reason not for saving less but for saving more. It may be, however, that the effect of uncertainty on savings depends on whether it is the uncertainty about future income or the uncertainty of gathering the fruits of one's savings.

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on Nigeria, namely, that in territories such as Nigeria the boundary between "production" and "living" should be drawn around the individual and not around the household.

Towards the end of the booklet there is a section based on a pertinent question—"whether the producers of national income data have streaked ahead of the consumers". The author stresses the use made of the calculations by "international agencies, branches of the metropolitan government, politicians and so on" and their value in refuting the arguments of partially informed critics of colonial policies. He is, however, of the opinion that "the run of the mill Colonial Administrator is unfamiliar with such devices and perhaps does not try to make himself familiar with them". He is confident (as he has every right to be, having had some able young administrators amongst his pupils) that this difficulty will disappear with the passage of time as the statistics improve and "a new race of more statistically conscious administrators emerges". There is much to be said for not making the excessive claims to be found in the work of some writers on national income topics.

W. F. SEARLE.

18.—*An Economic Survey of Northern Ireland.* By K. S. Isles and N. Cuthbert. Belfast, H.M.S.O., 1957. xxv, 646 pp. 9½″. 35*s*.

The survey, undertaken at the request of the Northern Ireland Government in 1946, had, as its practical objective, the assessment of Northern Ireland's economic condition and the examination of factors limiting its economic development. Being primarily an academic study, it was not within the scope of the investigation to make specific recommendations on what action should be taken either by the Government or by the public. Nevertheless, some important conclusions and inferences are drawn which will be of considerable use in determining future policy. The authors were given full access to the relevant official records and their analysis covers such matters as the general structure of industry, export trade, location, scale and specialization of industry, transport, investment and supply of capital, labour supply and costs, production, and economic interdependence with Great Britain.

Northern Ireland is made up of the six counties of Antrim, Armagh, Down, Fermanagh, Londonderry and Tyrone, having a combined population of some 1½ million. Several features make the Province unique within the United Kingdom. Firstly, it is separated from Great Britain by the Irish Sea; secondly, raw materials for industrial purposes are lacking; thirdly, the area suffers from under-employment; and fourthly, there is a separate Government which exercises powers affecting most aspects of the economy. The want of natural resources, combined with Northern Ireland's geographical position, are the basic causes for the persistent tendency for income to be relatively low and unemployment relatively high. The industries which have thrived best are those in which the transport costs form a relatively small proportion of the total cost. A limited success has been achieved by several industries where economies have been effected through lower costs of labour or cheaper rents. Other specialized firms producing for a sheltered domestic market have also been able to survive. But as the authors point out, "there are many industries, covering a wide range of products, in which conditions of competition are more severe and which cannot operate profitably in Northern Ireland at all—or only on a very limited scale—either because there are no offsetting economies of any significance or because the diseconomies are too large". In short, industrial expansion tends to be confined to a narrow range of industries whose transport costs are proportionately small.

This restriction in the economy and the high degree of specialization on a few industries leads in turn to insufficient investment which manifests itself in two ways. Firstly, people living in Northern Ireland invest a large part of their capital in Great Britain rather than at home and secondly, people in Great Britain invest a relatively small amount in Northern Ireland. The low level of domestic investment cannot be attributed to the low propensity of the people of Northern Ireland to save: in fact, the average saving out of income in Northern Ireland appears to be above that of Great Britain. Rather, the basic cause of the large net export of capital from Northern Ireland, and hence the relative inadequacy

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of the growth of capital for industry in the Province, is the low expectations of profit from domestic investments. The consequent tendency for Northern Ireland to suffer from a relative deficiency in employment is further aggravated by the somewhat faster growth of population than in Great Britain. Under-employment, therefore, is a permanent feature of the economy.

Several remedial measures are suggested. For instance, subsidies of various kinds could be granted by the government to make production relatively more profitable, or the government could invest in a programme of public works. Some firms have been deterred from establishing branches in Northern Ireland through the rise and relative uncertainty of transport costs. One solution to this would be for the government to operate a cross-channel shipping service of its own, the necessary legislative power for which already exists in the Transport Act (Northern Ireland), 1948. There is also evidence that some firms have under-estimated the potential advantages of operating in Northern Ireland with its good supply of cheaper labour. Again, convincing reassurance on Northern Ireland's constitutional position as part of the United Kingdom is important in attracting British industry and might, in itself, create a significant stimulus to economic development. As a means of giving effect to some of the proposals it is suggested a development corporation should be established to encourage and assist the promotion of new industrial ventures. It is a reflection on how seriously the survey is being taken that this body was created under the chairmanship of Lord Chandos before the actual publication of the report. This, however, is only one of many measures that could be taken by management, labour, and government to ameliorate the situation.

It is not possible within the confines of a short review to do more than touch on some of the problems and points examined in this report. There is no doubt that it will provide material for public discussion, political debates and academic research but, perhaps more important, the data should open the way for some clear thinking and realistic planning.

A. BARR.

19.—*Elementary Business and Economic Statistics*. By Alva M. Tuttle. New York and London, McGraw-Hill, 1957. xiii, 663 pp. 9″. 50*s*. 6*d*.

The author justifies putting yet another elementary statistical text-book on the market by his experience that none has yet proved entirely satisfactory for the course in business and economic statistics taken by all students of the College of Commerce and Administration of the Ohio State University. Having been written with this course in view the book is unlikely to be "entirely satisfactory" for similar courses in British colleges and universities. However, it may prove useful in many of them.

The text covers classification and collection of data; tables, graphs and diagrams; averages and dispersion; elementary sampling and statistical inference; index numbers; correlation; and analysis of time series. This seems satisfactory enough, but the emphasis is unlikely to be quite right. Survey methods receive scattered attention in the chapters on collection and sampling, and in an appendix of the American government's "Standards for Statistical Surveys". Tabular presentation alone gets as much space as survey methods and graphic presentation three times as much. The chapters on graphic presentation, however, are very clear and thorough and, in the reviewer's opinion, the best in the book. The chapter on sampling and statistical inference takes 50-odd pages and deals with the mean and a proportion. Time series analysis takes twice as many pages and deals only with separation and estimation of components when the observed time series is assumed to be their product; it does not even mention the possibility of assuming an additive model.

The book aims to cater for the non-mathematical student and in general it succeeds in this aim. In the chapter on averages the sections on assumptions made for the calculations for grouped data are particularly clear. But a correction is needed to the section on the short method of computing the arithmetic mean as the author implies that a working unit can be used only if the classes of a frequency distribution are all of the same width. Two appendices explain the use of the reciprocal in division, the calculation of square

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roots, and the use of logarithms and provide a very convenient reference for those who need one. Tables are also given. However, the author does forget his standard once or twice. On page 56 he mentions in passing that n classes can be arranged in n-factorial ways. Some references are made to explanations in later sections which can only confuse the student working systematically through the book. In the explanation of the short method of computing the arithmetic mean his notation allows him to write $X = \frac{-13}{100}$ and, six lines later, $X = 40{\cdot}45 + 3\frac{-13}{100}$. And a footnote claiming that there is no possibility of confusion is hardly sufficient justification for writing $t = t - 29{\cdot}5$.

The writer is likely to be too fond of definition for the taste of his British readers. The chapter on tabular presentation is the most annoying in this respect. For example, five lines are taken to define a statistical table, and names are given to six kinds of headings. While the practising business statistician may want to known these terms when dealing with his printer and a reference for them, or an appendix, would have been useful, the details can only make tedious reading and unnecessary learning for the student. In one to two cases, definition leads the author astray. "Statisticians have applied various terms to the aggregates under investigation, such as *population*, *variate*, *variable* or *universe* . . . In this book the various aggregates which are or may be under statistical investigation will be called categories". Following this definition the author fails to distinguish at the outset between the number of items in a group and the total of the values of a variate for the items in a group—a distinction which often causes students difficulty when they come to calculating arithmetic means. On page 61, a proportion is rather surprisingly defined as "a statement of equality between two ratios", but the word is used in its usual sense in the discussion of sampling and statistical inference.

The price of the book is a disadvantage and is no doubt attributable to its length.

RITA MAURICE.

20.—*Analysis for Production Management*. By E. H. Bowman and R. B. Fetter. Homewood (Ill.), Irwin, 1957. xiii, 503 pp. 9″. $6.50.

The authors of this remarkable book are both assistant professors in a school of industrial management. "One of the big problems in education for industrial management that must be resolved", they say in their preface, "is to bridge the sometimes large gap between the new insights, theories and methods constantly being developed in the management sciences and the needs, realities, and capabilities of the industrial manager. This book is offered as a small part of the bridge."

After an opening section on "orientation", which gives some historical background, a general discussion of production problems in the light of scientific method, and an outline of schematic models for production planning, including Gantt charts, the subjects discussed fall mainly in the fields of mathematical programming, statistical analysis and economic analysis.

The section on mathematical programming is concerned mainly with linear programming as applied to mixing, transport and purchasing problems. The simplex method is described and exemplified, and there is a brief mention of other programming methods, including dynamic programming.

The topics dealt with under statistical analysis are control charts (mean and range, fraction defectives, and defects per unit), sampling inspection (O.C. curves and A.O.Q. for single sampling schemes, Dodge-Romig double sampling, sampling inspection by variable, and a brief mention of sequential sampling) and industrial experimentation (one-, two- and three-factor analyses, with interactions).

The remaining section, called Economic Analysis, turns out to be a 100-page summary of operational research techniques applied to both stochastic and non-stochastic models. There is an account of the Monte Carlo method, exemplified by application to a problem

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of deciding maintenance policy for machine components, a discussion of waiting times, and various inventory and other problems.

The organization of the book has evidently been carefully planned. In each chapter the various techniques are first described in general terms, then illustrated by examples, then summarized; there follow a list of references and a set of fairly realistic problems for the reader. This is not all: the sections described above are followed by a final substantial section devoted to "cases", thinly disguised accounts of some ten real companies with their production and control processes, plant layout, storage and transport facilities and problems, costs and so on, all described and discussed in some detail.

Finally there is an appendix giving a glossary of symbols, and nine useful statistical tables. After all this it is perhaps superfluous to add that there is an index.

In a work of this sort it would be ungracious to criticize the authors for omissions (though one might perhaps have expected a reference to Box's work on optimization by steepest ascents); rather should one congratulate them on their astonishing comprehensiveness: a company whose managers were masters of the contents of this book would, one feels, be formidable.

E. H. LLOYD.

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STATISTICAL NOTES

(1) British Official Statistics

The index of retail prices compiled by the Ministry of Labour and National Service which was 107 in July 1957 fell to 106 in August and September and rose to 107 in October and 108 in November. The fall in August, which was only fractional, was largely the result of a decline in the prices of potatoes, sugar and bacon, though there were some increases in the price of coal, electricity and gas. In October there were increases in rents and in the prices of newspapers and higher postal and telephone charges. There were further increases in rents and in the price of coal in November. The detailed figures of the weights used in calculating the index and the indices for different commodity groups were as follows:

	Food	*Alcoholic Drinks*	*Tobacco*	*Housing*	*Fuel and Light*	*Durable Household Goods*	*Clothing and Footwear*	*Transport and Vehicles*	*Miscellaneous*	*Services*	*All Items*
Weights:	350	71	80	87	55	66	106	68	59	58	1,000
July 16th, 1957	108·4	104·1	105·3	110·0	106·3	100·6	102·1	109·0	107·0	108·4	106·6
Aug. 13th, 1957	106·9	104·7	105·3	110·2	108·1	100·6	102·3	109·2	107·1	109·2	106·4
Sept. 17th, 1957	104·8	104·7	107·8	110·5	109·2	100·7	102·7	110·1	107·2	110·8	106·1
Oct. 15th, 1957	104·7	105·7	107·8	114·1	109·9	101·2	102·9	111·6	111·1	113·7	107·1
Nov. 12th, 1957	104·8	105·7	107·8	115·9	115·3	101·3	103·0	112·7	111·1	114·0	107·7

The Ministry of Labour index of weekly wage rates, calculated on the basis of January 31st, 1956 = 100, showed that in July 1957, the level was 111·1. It rose to 111·4 in August, 111·5 in September, 111·8 in October and 111·9 in November. In manufacturing industries alone the figures were 111·2, 111·7, 111·8, 111·9 and 112·0. The principal classes of workers affected by increases included cotton operatives, iron and steel workers, employees in catering establishments, boot and shoe operatives, Post Office manipulative workers, agricultural workers and employees in sugar confectionery making, hosiery, milk distribution, furniture making and textile bleaching and dyeing.

The six-monthly enquiry into average earnings showed that in April 1957, the average earnings per week in the industries covered by the enquiries, i.e. the manufacturing industries, and some of the principal non-manufacturing industries, were 241*s.* 6*d.* for men, 125*s.* 10*d.* for women, 105*s.* 0*d.* for boys and youths and 83*s.* 11*d.* for girls. These figures represent increases since April 1947, of 96, 87, 122 and 109 per cent. For all workers combined the average was 204*s.* 7*d.*, an increase of 98 per cent. This compares with an estimated rise in weekly full-time *rates* of wages in the same industries of about 69 per cent. In manufacturing industries alone the average weekly earnings in April 1957 were 248*s.* 11*d.* for men, and 126*s.* 3*d.* for women. The corresponding hourly earnings were 62·1*d.* and 36·5*d.* The average weekly hours worked in all the industries and services remained at 46·6.

The Ministry of Labour has begun to publish an index of normal weekly hours. On the basis of June 30th, 1947 = 100, the index for the end of January 1956 was 98·0 If the latter figure is equated to 100, the index for August 1957 is 99·9. In a descriptive article in the September issue of the *Ministry of Labour Gazette* figures are given from which a continuous index back to 1920 can be calculated.

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The total working population and the numbers in civil employment in the five months ended October 1957, were as follows:

(Thousands)

Date	Total Working Population			Numbers in Civil Employment		
	Males	*Females*	*Total*	*Males*	*Females*	*Total*
June, 1957	16,105	7,857	23,962	15,247	7,772	23,019
July, 1957	16,120	7,865	23,985	15,263	7,777	23,040
Aug., 1957	16,155	7,902	24,057	15,302	7,809	23,111
Sept., 1957	16,145	7,912	24,057	15,296	7,816	23,112
Oct., 1957	16,141	7,920	24,061	15,287	7,818	23,105

The number of persons on the unemployment registers of the Employment Exchanges rose by 21,300 in August, 1,800 in September, 7,800 in October and 41,300 in November. The total for November represented 1·5 per cent. of the total number of employees in Great Britain. The percentages in the different Regions ranged from 0·9 in the North Midlands, to 2·6 in Scotland and 2·9 in Wales.

The following is the sex analysis of the figures:

Number of Unemployed Persons on the Registers of Employment Exchanges

	Men and Boys	*Women and Girls*	*Total*
July 15th, 1957	171,446	72,860	244,306
Aug. 12th, 1957	185,577	80,050	265,627
Sept. 16th, 1957	183,968	83,438	267,406
Oct. 14th, 1957	188,855	86,367	275,222
Nov. 11th, 1957	218,968	97,555	316,523

Of the total of 316,523 in November, 87,254 had been unemployed for less than two weeks, 95,286 for two to eight weeks, and 119,317 for over eight weeks, while 14,666 were temporarily stopped. In the four weeks ended November 6th, 140,972 vacancies were filled by the Employment Exchanges. The number of vacancies unfilled at this date was 251,935.

The number of insured persons absent from work owing to sickness, including self-employed as well as employed, were 793,200 in July, 773,600 in August, 929,800 in September, 1,494,300 in October and 959,100 in November. The relatively high figures in the last three months reflect the effects of the influenza epidemic. The number of employed persons absent owing to industrial injuries were 56,000, 49,000, 53,400, 50,400 and 52,000.

Figures published in the *Ministry of Labour Gazette* for December 1957, show the number of members of trade unions in 1956 as 9,700,000 in 647 unions. Of this total 6,521,000 were members of 17 unions each with a total of 100,000 or more, while another 1,266,000 were in unions with a membership of 50,000 to 100,000.

Scottish Agricultural Economics Vol. 8 (H.M.S.O. Edinburgh 1957, 62 pages, 3*s.* net) is the latest in a regular annual series dealing with the results of investigation and research work carried out by the Economics Staff of the Department of Agriculture for Scotland.

Many of the articles are continuations of those published in previous volumes, and cover estimates of Scottish Agricultural Output (including preliminary results for 1956–57, gross and net at current and constant prices), bank advances to Scottish agriculture, and

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the annual surveys of financial accounts, of milk costs and of farm workers' earnings. All these form a useful supplement to the two parts of *Agricultural Statistics, Scotland*, which deal in detail with the basic agricultural statistics on acreage, livestock numbers, prices, etc.

Other articles concentrate on particular aspects of the information obtained either from the annual surveys, or from intensive analyses of the agricultural census data, or as by-products of other official or semi-official statistics. Some of these are current versions of series published in one or more of the earlier volumes.

This eighth volume also contains a special study which sets out some background facts about the economic position of the small farm in Scotland. This provides an interesting illustration of the type of material which can profitably be extracted from agricultural economic statistics.

(2) Other Statistics

Energy Consumption in the Manufacturing and Mining Industries of Canada, Selected Years—1926 *and* 1953. (*Reference Paper No.* 73: 1957 *of the Dominion Bureau of Statistics, Ottawa, Canada*). This paper is one of three related studies in energy use in Canada. The first study, Paper 69, dealt with commodity accounts for 1948 to 1953, whilst the third—giving similar accounts for earlier years—is promised in due course.

The present paper brings together data on the consumption of fuel and electricity by mines and manufacturers in Canada, together with data on any electricity produced by them. Data are shown for Canada as a whole and for each province for selected years between 1926 and 1953.

In all, some 48 tables are provided and embody some information not previously published. They give information in terms of values and quantities of fuel and electricity used. For example, the table for Canada as a whole shows the tonnage of bituminous, anthracite and other coals and coke consumption, together with volumes of gasoline, kerosene, gas and electricity consumed, whilst the succeeding table shows the values of these fuels for the selected years covered.

A short interpretation of the data describes the general picture of fuel consumed by manufacturing industry in Canada. This includes a brief analysis of the proportion of the input of energy by manufacturing industry and mines attributable to various fuels and electricity in 1948.

CURRENT NOTES

Fellows will wish to congratulate Mr. J. R. H. Shaul, who has recently been appointed the first Honorary Research Fellow of the University College of Rhodesia and Nyasaland, for his contributions to scholarship and his services in establishing the Statistical Service of the Federation.

Fellows will be pleased to know that Mr. Herbert Tetley, C.B., M.A., F.I.A., a member of the Council of the Society, has been appointed to succeed Sir George H. Maddex, K.B.E., on his retirement from the position of Government Actuary as at March 31st, 1958.

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OBITUARY

Roy Goncalves Glenday, 1889–1957

Roy Glenday, a member of the Council until last June, died in August, 1957. He was first elected to the Council in 1934 and at the time of his death had served altogether for 15 years over four periods. He was a Vice-President in 1943–45. Almost from its foundation, towards the end of the 1914–18 war, he had been Economic Adviser to the Federation of British Industries, retiring from that post in 1954. He was probably the first man to hold such a position in any organization of British manufacturers. It was a particularly difficult position, not so much because of its pioneering aspect—economists and statisticians were practically unknown to British industry before the 1914–18 war—but because of its wide field, covering the interests of all industry in general. To serve as an economic adviser a single industry, in which the interests of those in it are fairly homogeneous, is an entirely different matter from serving a heterogeneous organization covering every industry, in which the point of view, for example, of those concerned with the production of consumer goods is on a number of questions the reverse of the point of view of those engaged in the production of capital goods. Glenday's patience and good humour greatly helped him in his task. The picture of him waiting for the opportunity of making a positive contribution to a discussion, but biding his time and listening to what he no doubt frequently felt was rank heresy, will be familiar to many who had occasion to attend F.B.I. meetings.

At Cambridge he took both the Natural Science and the Law Tripos, and when war broke out in 1914 he was chief research chemist at the Magadi Soda Company in Kenya. He served throughout the war in East Africa, first as a trooper and subsequently being commissioned in the King's Africa Rifles. He was mentioned in despatches, obtained the M.C., was seriously wounded and lost his right arm. In spite of this disability he played a fair game of golf and was a regular winter visitor to the Alps for ski-ing.

He was called to the Bar (Middle Temple) in 1918 but did not practise, becoming immersed in the task of assisting in the formation and consolidation of the staff of the F.B.I. In his capacity as Economic Adviser he became a familiar figure as one of British industry's representatives at innumerable conferences in Europe, and attended the famous Ottawa conference. He was not a fully equipped statistician. While he readily appreciated the importance of statistics regularly collected and intelligently tabulated in indicating trends and changes in economic factors—he was responsible for the quarterly "Business Barometer" circulated by the Federation—he was sceptical of the value of some of the 'paraphernalia surrounding statistical analysis.' He was, for example, an adverse critic of a good deal of the early work on comparisons of industrial productivity in different countries.

He read the following papers before the Society:

1932—"Business forecasting" (this *Journal*, Vol. 95, Part 1);
1935—"The use and misuse of economic statistics" (this *Journal*, Vol. 98, Part 3);
1938—"Long-period economic trends" (this *Journal*, Vol. 101, Part 3);

and also opened a discussion in 1942 on "Economic reconstruction after the war" (this *Journal*, Vol. 105, Part 1). He also published three books—*Passing of Free Trade* (1930); *The Economic Consequences of Progress* (1933) and *The Future of Economic Society* (1944).

E. C. Snow.

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Vol. 11 (1957)—Sull'impiego dei modelli econometrici nei piani economici: *G. Cusimano.* Nuovi contributi all'analisi statistica dello stile letterario: *F. Galantino.* Sul metodo dei probits: *G. Cusimano.* Di alcune combinazioni lineari ottenuti con il metodo dei minimi quadrati: *C. Ferreri.*

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No. 1 (1957)—The separation of ownership and management in the large corporations in Japan: *K. Urabe.* An analysis of securities market in Japan: *K. Fukuda.* Small wholesalers in the cotton textiles marketing in Japan: *Y. Arakawa.*

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YUGOSLAVIA—

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mechanical smoothing methods: *I. Lah* (Serbo-Croat text, English summary). Ménages et familles dans le recensement de 1953; *M. Macura* and *D. Breznik* (Serbo-Croat text, French summary).

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LIST OF ADDITIONS TO THE LIBRARY

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BANK OF ENGLAND WEEKLY RETURNS

For Wednesday in each Week, during the year 1957

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BANK OF ENGLAND

For Wednesday in each Week,

(000's omitted)

1	2	3	4	5	6	7	8		9
ISSUE DEPARTMENT									
Liabilities			Assets						
Notes in Circulation	Notes in Banking Department		Govt. Debt. (£11,015) and Other Govt. Securities	Other Securities	Coin other than Gold Coin	Total of cols. 4, 5 and 6 (Fiduciary Issue)	Gold Coin and Bullion†		Total of Liabilities or Assets
								per oz. fine	
£	£		£	£	£	£	£	*s. d.*	£
1,968,912	56,448	Jan. 2	2,021,211	777	3,012	2,025,000	361	*250/7*	2,025,361
1,928,391	46,968	,, 9	1,971,281	704	3,015	1.975,000	360	*250/0*	1,975,360
1,889,765	35,594	,, 16	1,921,263	726	3,010	1,925,000	359	*249/3*	1,925,359
1,867,344	58,015	,, 23	1,921,189	801	3,010	1,925,000	358	*249/0*	1,925,358
1,861,616	63,743	,, 30	1,921,240	749	3,011	1,925,000	359	*249/2*	1,925,359
1,863,143	62,216	Feb. 6	1,921,255	734	3,011	1,925,000	359	*249/3*	1,925,359
1,862,099	63,260	,, 13	1,921,280	709	3,011	1,925,000	359	*249/6*	1,925,359
1,859,150	66,209	,, 20	1,921,276	710	3,015	1,925,000	359	*249/4*	1,925,359
1,873,368	51,990	,, 27	1,921,187	799	3,014	1,925,000	359	*249/3*	1,925,359
1,886,207	39,153	Mar. 6	1,921,213	772	3,015	1,925,000	359	*249/7*	1,925,359
1,890,705	34,655	,, 13	1,921,253	735	3,012	1,925,000	359	*249/8*	1,925,359
1,894,032	31,328	,, 20	1,921,187	800	3,013	1,925,000	360	*250/3*	1,925,360
1,902,831	22,529	,, 27	1,921,271	714	3,015	1,925,000	360	*250/4*	1,925,360
1,915,159	10,202	April 3	1,921,192	798	3,011	1,925,000	360	*250/5*	1,925,360
1,926,801	48,559	,, 10	1,971,252	736	3,012	1,975,000	360	*250/3*	1,975,360
1,945,198	30,162	,, 17	1,971,230	757	3,013	1,975,000	360	*250/4*	1,975,360
1,952,213	23,146	,, 24	1,971,271	715	3,014	1,975,000	360	*250/1*	1,975,360
1,952,203	23,157	May 1	1,971,201	786	3,014	1,975,000	360	*250/2*	1,975,360
1,948,586	26,774	,, 8	1,971,281	706	3,014	1,975,000	360	*250/2*	1,975,360
1,947,762	27,598	,, 15	1,971,210	780	3,011	1,975,000	360	*250/4*	1,975,360
1,947,813	27,548	,, 22	1,971,213	775	3,012	1,975,000	360	*250/4*	1,975,360
1,953,671	21,689	,, 29	1,971,195	792	3,013	1,975,000	360	*250/4*	1,975,360
1,966,089	34,272	June 5	1,996,282	704	3,015	2,000,000	361	*250/9*	2,000,361
1,976,612	23,749	,, 12	1,996,216	769	3,014	2,000,000	361	*250/10*	2,000,361
1,980,473	19,888	,, 19	1,996,286	703	3,011	2,000,000	360	*250/6*	2,000,360
1,985,487	14,874	,, 26	1,996,258	728	3,014	2,000,000	361	*250/9*	2,000,361
1,995,437	54,924	July 3	2,046,288	702	3,010	2,050,000	361	*250/7*	2,050,361
2,013,691	36,670	,, 10	2,046,220	766	3,014	2,050,000	361	*250/9*	2,050,361
2,023,344	27,017	,, 17	2,046,287	703	3,010	2,050,000	361	*251/0*	2,050,361
2,040,238	10,123	,, 24	2,046,222	766	3,013	2,050,000	361	*251/3*	2,050,361
2,059,474	15,888	,, 31	2,071,283	705	3,012	2,075,000	362	*251/4*	2,075,362
2,055,195	20,167	Aug. 7	2,071,204	784	3,012	2,075,000	362	*251/4*	2,075,362
2,037,063	38,299	,, 14	2,071,228	757	3,015	2,075,000	362	*251/4*	2,075,362
2,006,983	18,378	,, 21	2,021,238	751	3,011	2,025,000	361	*251/0*	2,025,361
1,995,479	29,882	,, 28	2,021,283	705	3,012	2,025,000	361	*251/3*	2,025,361
1,988,928	36,433	Sept. 4	2,021,216	773	3,010	2,025,000	361	*251/2*	2,025,361
1,983,782	16,579	,, 11	1,996,285	705	3,011	2,000,000	361	*251/3*	2,000,361
1,976,045	24,316	,, 18	1,996,224	764	3,012	2,000,000	361	*251/2*	2,000,361
1,967,498	32,862	,, 25	1,996,282	707	3,012	2,000,000	360	*250/3*	2,000,360
1,970,930	29,430	Oct. 2	1,996,219	767	3,014	2,000,000	360	*250/3*	2,000,360
1,973,019	27,340	,, 9	1,996,284	705	3,011	2,000,000	359	*249/8*	2,000,359
1,969,726	30,633	,, 16	1,996,209	780	3,011	2,000,000	359	*249/7*	2,000,359
1,966,081	34,278	,, 23	1,996,285	705	3,010	2,000,000	359	*249/7*	2,000,359
1,966,967	33,392	,, 30	1,996,206	780	3,014	2,000,000	359	*249/10*	2,000,359
1,976,289	24,071	Nov. 6	1,996,208	778	3,014	2,000,000	359	*249/9*	2,000,359
1,983,513	16,847	,, 13	1,996,196	794	3,010	2,000,000	359	*249/8*	2,000,359
1,987,036	13,323	,, 20	1,996,282	704	3,014	2,000,000	359	*249/2*	2,000,359
2,001,449	48,910	,, 27	2,046,285	704	3,011	2,050,000	359	*249/2*	2,050,359
2,034,018	16,340	Dec. 4	2,046,269	721	3,010	2,050,000	358	*249/1*	2,050,358
2,080,870	19,489	,, 11	2,096,212	776	3,013	2,100,000	359	*249/6*	2,100,359
2,118,574	31,785	,, 18	2,146,264	724	3,012	2,150,000	359	*249/6*	2,150,359
2,127,954	22,405	,, 25	2,146,205	785	3,011	2,150,000	359	*249/7*	2,150,359

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WEEKLY RETURNS*

during the year 1957

(000's omitted)

10	11	12	13	14	15	16	17	18	19
BANKING DEPARTMENT									
Liabilities					Assets				
Capital (£14,553) and Rest	Deposits: Public‡	Deposits: Bankers	Deposits: Other Accounts		Govt. Securities	Discounts and Advances	Other Securities	Reserve (Notes and Coin)	Total of Liabilities or Assets
£	£	£	£		£	£	£	£	£
18,158	17,968	212,364	75,340	Jan. 2	203,222	45,262	16,964	58,382	323,830
18,194	18,510	219,869	74,323	,, 9	253,947	10,627	17,420	48,901	330,895
18,231	13,668	218,986	73,279	,, 16	259,322	9,972	17,334	37,537	324,164
18,328	14,779	212,875	71,834	,, 23	200,457	39,692	17,701	59,967	317,816
18,332	14,428	232,111	76,037	,, 30	222,372	34,902	17,934	65,702	340,909
18,364	14,058	228,030	73,132	Feb. 6	229,262	22,487	17,641	64,194	333,583
18,398	16,580	225,258	72,696	,, 13	218,602	30,937	18,138	65,255	332,932
18,427	13,400	233,848	72,635	,, 20	219,817	31,687	18,560	68,246	338,310
18,455	13,500	225,164	73,749	,, 27	242,897	15,822	18,133	54,016	330,867
18,478	11,754	217,467	71,288	Mar. 6	224,237	33,626	19,860	41,265	318,988
18,499	13,562	223,146	71,995	,, 13	248,592	21,735	20,062	36,813	327,202
18,515	15,848	222,055	71,992	,, 20	249,977	24,626	20,279	33,528	328,410
18,531	13,734	228,900	72,529	,, 27	269,497	18,600	20,799	24,798	333,694
18,553	10,786	216,997	71,806	April 3	240,902	44,765	19,964	12,511	318,141
17,705	20,118	210,657	71,838	,, 10	198,927	51,570	18,913	50,907	320,317
17,756	16,884	214,595	74,766	,, 17	247,507	24,965	18,986	32,543	324,001
17,827	11,527	202,436	74,491	,, 24	240,202	21,500	19,006	25,573	306,281
17,831	11,801	219,541	74,010	May 1	243,392	35,255	18,936	25,600	323,182
17,905	9,153	210,771	77,281	,, 8	202,042	64,979	18,874	29,216	315,111
17,913	10,374	207,797	73,903	,, 15	207,312	53,578	19,060	30,036	309,986
17,951	10,664	206,379	73,796	,, 22	217,547	42,270	19,003	29,971	308,791
17,963	10,123	204,405	75,305	,, 29	224,102	40,530	19,061	24,105	307,797
18,011	13,874	211,228	70,643	June 5	212,882	45,100	19,088	36,687	313,757
18,025	25,717	215,052	73,924	,, 12	260,782	26,965	18,799	26,172	332,718
18,074	17,000	208,509	71,720	,, 19	250,715	23,305	18,955	22,329	315,304
18,109	12,215	216,261	71,801	,, 26	249,262	32,625	19,180	17,318	318,385
18,149	14,471	223,180	74,271	July 3	202,172	51,480	19,044	57,376	330,071
18,188	18,586	203,896	73,833	,, 10	238,057	18,330	18,996	39,121	314,503
18,257	10,954	213,746	73,334	,, 17	242,984	24,805	19,028	29,474	316,291
18,297	10,975	208,474	75,665	,, 24	257,949	24,135	18,768	12,559	313,411
18,303	13,364	205,015	73,889	,, 31	243,983	29,921	18,368	18,299	310,571
18,334	12,998	195,022	73,543	Aug. 7	233,009	26,112	18,183	22,592	299,897
18,375	12,884	203,017	72,951	,, 14	210,878	37,657	17,962	40,729	307,226
18,416	11,740	209,519	73,844	,, 21	247,596	27,216	17,889	20,818	313,519
18,450	11,603	199,280	74,164	,, 28	235,286	17,641	18,248	32,322	303,497
18,480	13,314	205,205	72,052	Sept. 4	229,556	21,606	19,017	38,874	309,052
18,505	16,985	217,288	72,827	,, 11	267,431	19,331	19,815	19,030	325,605
18,513	11,077	232,133	71,672	,, 18	267,021	19,336	20,263	26,776	333,395
18,532	12,961	216,843	73,111	,, 25	251,196	15,081	19,843	35,327	321,447
18,551	12,380	224,319	76,964	Oct. 2	249,066	31,226	20,035	31,889	332,215
17,688	15,992	226,537	73,653	,, 9	269,444	14,291	20,332	29,804	333,870
17,725	11,591	239,677	74,286	,, 16	270,516	18,536	21,134	33,093	343,279
17,820	10,698	227,657	75,039	,, 23	255,250	17,040	22,177	36,748	331,215
17,825	10,133	234,613	75,671	,, 30	265,784	13,695	22,913	35,851	338,242
17,850	9,338	228,343	73,155	Nov. 6	248,648	30,070	23,446	26,522	328,686
17,883	11,924	223,635	74,090	,, 13	263,591	20,795	23,850	19,295	327,531
17,970	10,480	228,227	75,658	,, 20	269,126	23,355	24,102	15,753	332,336
17,973	10,123	226,873	76,440	,, 27	236,416	19,770	23,895	51,328	331,409
18,019	10,571	208,233	73,611	Dec. 4	247,036	21,217	23,436	18,745	310,434
18,059	16,024	199,190	75,958	,, 11	242,446	21,902	23,002	21,880	309,230
18,078	11,011	200,199	80,525	,, 18	223,646	29,102	22,906	34,159	309,813
18,117	9,836	199,481	81,936	,, 25	241,441	20,982	22,181	24,766	309,370

* Bank Rate was 5½% on 1st Jan., 5% from 7th Feb., 7% from 19th Sept.
† Valuation, per oz. fine, given in italics.
‡ Including Exchequer, Savings Banks, Commissioners of National Debt, and Dividend Accounts.

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Classification of the Population by Social and Economic Characteristics

The French Experience and International Recommendations

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[Read before the Royal Statistical Society, January 13th, 1958, the President, Sir Harry Campion, C.B., C.B.E., in the Chair]

I. The Traditional Occupational Classifications

1. It is essential to be thoroughly acquainted with the structure of the economically active population of a country. Various particular investigations help to obtain interesting data on certain questions (for example: labour statistics collected by trade union organizations, periodical surveys by sampling on the level of employment), but the general censuses of population effectively form the most complete and the best documented source of information, which furnishes comparable overall details and which provides illustration in bold relief, on a given date, of the multiple aspects of the problem. The comparison of the successive results of censuses—as far as the methods and nomenclatures remain by themselves comparable—provides the means for following up closely the evolution of the principal factors.

2. For a long time, two fundamental notions were brought to the fore for the purpose of characterizing the active population, the "Industry" and the "Occupation". The Industry is a criterion linked, not to the persons themselves, but to the *establishments or enterprises* where they work; it indicates the nature of the services rendered or of the products extracted, manufactured, treated or sold in the establishment or enterprise, for example:

Agriculture.
Coal mining.
Manufacturing of motor-cars.
Road transport.
Wholesale grocery.
Education.
Financial administration.

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The *occupation* characterizes the person himself or herself, it indicates the *profession* or *craft* learned and exercised by the individual, it is for example:

Farmer.
Miner.
Carpenter.
Locomotive driver.
Unskilled labourer.
Doctor.
Administrator.

These two concepts, Industry and Occupation, are different and it is important to distinguish the one from the other. Certainly there does exist a link between them; it is evident that generally speaking farmers are to be found in agriculture, the locomotive driver in railway transport. But on the other hand, you can meet within the body of the same industry numerous occupations, for example: in a factory manufacturing cars you will find different mechanical occupations (turners, millers, fitters, coach-builders, etc. . . .) and perhaps even masons, carpenters and house-painters (for the maintenance of buildings of the enterprise itself), multi-copying machine operators (if the enterprise has an equipment of machines with punched cards), etc. On the other hand, there are certain occupations which are common to numerous industries (especially the office occupations: draughtsmen, typists, telephone operators).

This is the reason why it is indispensable that the census provides *separate* data, corresponding to each of these two concepts; but it is also useful to have cross-tabulation of industry and occupation, showing clearly the internal distribution of each criterion.

II. Necessity for a New Classification

3. The two preceding concepts, cross-tabulated if necessary with other particulars such as sex, age, etc. . . . , are most useful for numerous studies of an economic and social nature (production, labour problems, professional training, etc. . . .). But they do not cover all aspects of occupation and they prove to be insufficient when it is intended to study scientifically the *behaviour of persons on the economic and human level in relation to their occupational and social situation.*

In particular, they do not facilitate the separation of categories corresponding to the common notion of "social class"; and—it is a fact of experience—in reality the population splits itself into groups which are distinguished through their particular attitudes and behaviours in various fields: in demography (number of children, frequency of divorces); in economy (quantitative and qualitative consumption, savings); in cultural matters (use of leisure hours, reading); and more generally in all the studies connected with sociology in an extended sense.

It is not necessary to insist on the evident existence of these groups, which are to be found in most countries, where they are designated by words of a current vocabulary: employers, owners, employees, workmen, staff, officials, farmers, merchants, retired persons, etc. . . . It is, however, as far as the statistician is concerned, also a fact of experience that numerous users of statistics, especially those relating to the elaboration of sampling plans for the purpose of making the most diverse probing inquiries (market

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surveys, opinion inquiries, election results, etc.) express the need for the splitting up of the population on these lines.

4. This is why, in recent censuses, the majority of countries have endeavoured to satisfy this need by making a wider analysis of the structure of the economically active population. Consequently, and in particular, a supplementary classification, the "employment status" was introduced, which makes a distinction between the following:

Employers (owners of establishments who employ persons drawing salaries or wages).

Own account workers (owners of establishments who do not employ persons drawing salaries or wages).

Employees.

Family workers (persons who work in a family enterprise, who are lodged and fed and who receive a remuneration which is generally lower than that of a person drawing a full salary).

In this manner, if we are to consider for example a butcher's establishment held by an owner who employs a butcher's boy and his own son, all the three belong to the same industry (retail butcher's trade) and exercise the same occupation (butcher) but there is a definite distinction between them under the *employment status*: the first will be the employer, the second the employee, and the third, the family worker—and this is important for the study of differences in their behaviour.

In this connection, we see that in certain countries, various supplementary criteria have been put into use, always in the same spirit of approaching more closely the economic and social reality, for example:

For the employers, the distinction between the "*large*" and the "*small*" employers according to the number of salaried or wage-earning persons of the enterprise or according to the area of the agricultural exploitation.

For the farmers, the distinction between the *owners* (who possess, at least a large portion of their lands and their means of production), the *tenants* (who hire the lands which they cultivate but in general are the proprietors of the livestock, the farming equipment, as well as the harvests), the sharecroppers (who do not possess either the land, the livestock or the implements and share the harvests with the proprietors).

The *economic sector*: private enterprises, public services and state enterprises and, in a few countries, producers' co-operatives (associations of producers working jointly, the means of production being in the majority of cases under joint property and the profits shared essentially commensurate with the work performed by each of the members).

The criterion "*liberal professions*" which groups the doctors, lawyers, architects, etc.

For the salary and wage earners, the distinction between the *non-manual workers* and the *manual workers*.

For the salary earners also, the differentiation amongst the employees of the senior salaried workers, the intermediate salaried workers, the subordinate employees, and amongst the manual workers the foremen, the skilled workers, the semi-skilled workers, the unskilled workers, and the service workers (domestic services, house-porters, service personnel of hotels etc. . . .).

It will be noticed that these various distinctions (employment status and additional criteria) do not spring from a uniform concept, but involve several criteria: link with the

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enterprise, size of the enterprise, ownership of the means of production, economy sector, nature of the work performed, seniority and skill.

5. Various trials carried out by different countries have revealed that effectively good correlations are obtained with the socio-economic behaviour of individuals by using a restricted classification derived from the main occupational characteristics, which have been indicated above, and which can be easily obtained through questionnaires, especially those of the census. To be more precise, it has appeared that the characteristic which gave the best results was the *employment status incorporating certain complementary criteria indicated above.*

If the majority of the additional distinctions can be obtained from information connected with the industry or the occupation of individuals (for example, the knowledge of the occupation generally permits the distinction of farmers, clerks and workmen, liberal professions, intermediate salaried employees, skilled manual workers, etc.) we can deduce that generally speaking it is possible to draw up a new classification for studies of socio-economic behaviour, based essentially on the following three characteristics:

Employment status.
Occupation.
Industry.

And, in certain instances, a few additional characteristics (size of the enterprise, ownership of the means of production, economic sector).

III. The French Socio-professional Classification

6. It is effectively in this manner that a few years ago the "*socio-professional classification*" was elaborated in France. It is reproduced below (the part concerning the economically *active* population). The classification is set forth in two components, the "socio-professional categories" (appropriately named) to which corresponds a 2-digit code and the "status" to which corresponds a single-digit code. Only certain well defined statuses can correspond to a given category, a factor which limits the number of possible groups.

In accordance with the nature of the studies, and the degree of precision of the data, the following may be used:

The detailed classification in categories and status.
The categories only or the status only.
The socio-professional "groups" corresponding to the first figure of the code.
Or any intermediate combination.

Code of Socio-professional Categories (France)

Socio-professional Groups and Categories

0. FARMERS—
   01. Owners.
   02. Farmer tenants.
   03. Sharecroppers.

1. AGRICULTURAL WORKERS—
   10. Agricultural workers.

2. EMPLOYERS IN INDUSTRY AND COMMERCE—
   21. Industrials.
   22. Artisans.
   23. Fishermen.
   26. Large merchants.
   27. Small merchants.

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3. Liberal Professions and Senior Personnel—
   30. Liberal professions.
   32. Literary and scientific professions.
   33. Engineers (private sector).
   34. Senior administrative personnel (private sector).
   35. Senior personnel (public sector).

4. Intermediate Personnel—
   41. Teachers. Medical and social services (private sector).
   42. Teachers. Medical and social services (public sector).
   43. Technicians (private sector).
   44. Intermediate administrative personnel (private sector).
   45. Intermediate personnel (public sector).

5. Non-manual Workers—
   51. Office employees (private sector).
   52. Office employees (public sector).
   53. Sales workers.

6. Manual Workers—
   60. Foremen (private sector).
   61. Skilled workmen (private sector).
   62. Skilled workers and foremen (public sector).
   63. Semi-skilled workers (private sector).
   64. Semi-skilled workers (public sector).
   65. Miners.
   66. Sailors and fishermen.
   67. Apprentice workers.
   68. Unskilled workers.

7. Personal Service—
   70. Servants (resident).
   71. Housekeepers (paid, non-resident).
   72. Other personal service.

8. Other Categories—
   80. Artists including musicians.
   81. Clergy.
   82. Army and police.

Status

0. Special status.*
1. Independents excluding salary earners.
2. Employers.
3. Family workers.
4. Apprentices.
5. Operatives of private establishments.
6. Household workers.
7. Operatives of public services.
8. State employees and employees of local authorities.
9. Unemployed.

* Number used in special cases, for example for the clergy.

IV. *Characteristics of the French Classification*

7. The French categories were established without resorting to any rigid principle, but aim at building up *a small number of groups as far as possible homogeneous in demographic, economic and social characteristics.*

The classification was based on experience acquired from previous studies and upon the needs which were most frequently expressed by the users, after having found out that their demands often presented numerous common points. It is a purely pragmatic work, but it has been proved by facts, as will be seen further on, for we have effectively found

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good correlations with numerous other factors. Briefly, Mr. J. Porte, who was the author of this classification, empirically sought, "amongst all the professional classifications possible, the one which would give the strongest correlations with the most variegated characteristics of the persons classified."

8. The classification is not linked in a precise manner to any *sociological theory*. The criterion of the "social classes" has served as a substratum, but in a rather flexible manner. However, it could not be otherwise; the sociologists have issued numerous theories often contradictory as regards the social classes.

9. The French classification does not pretend to be *perfect*; but this is true of all socio-professional classifications; the classification which will give the best correlation with, for example, political opinion, is not necessarily the one which will give the best correlation with the expenditure pattern or habits of listening in on the radio. It must therefore be admitted that a classification, valid for most purposes, is inevitably *imperfect for each of these purposes*. It is, however, necessary to have such a classification in order to be able to compare the results of the most diverse surveys, prepare *a priori* stratifications for the carrying out of investigations by sampling, etc. . . . ; furthermore, this classification should be one of the results obtained at the census of the population, and it is out of the question, for reasons of cost, to burden the census with too numerous criteria.

The classification used should therefore be a *compromise* between the exigencies of very different needs. On the practical plane, this leads to making a provision for a number of "basic categories", more numerous than is in general necessary in surveys, but this will permit *different re-groupings*, adapted to each survey. The flexibility of this process is quite convenient.

10. From the foregoing considerations, we deduce that it is not possible to give an *abstract definition* of socio-professional categories. There is no "socio-professional criterion", but a complex of criteria, used, however, in a different manner in the various parts of the classification. The classification necessarily appears as a *list of basic categories*, the contents of each category being clearly defined, as carefully as possible, consistent with the essential characteristics of an occupational order.

11. It will be finally noticed that the classification includes a *medium number of items*; for this also, experience has shown that we should seek for a *compromise* between great detail—inconvenient for use and necessarily including insignificantly small groups—and not enough detail, which necessarily reduces homogeneity. In both extreme cases, the classification entirely lacks interest.

12. One is liable to wonder that other important characteristics, which have a clear and sometimes a preponderating influence on the socio-economic behaviour of individuals, for example the income or education, were not used in the constitution of socio-professional categories. It is for the following reasons that these characteristics were not retained.

It is not possible, in France, and in numerous other countries, to ask questions on the nature and on the amount of *incomes* (incomes in general, whether it is question of salaries, unearned income, gains, profits, rents, reliefs etc.) as experience has shown that this would raise numerous difficulties. This is to be regretted, on account of the obvious interest which such precise data on this subject would present. But in addition to the difficulty of the question itself, which would first of all necessitate quite precise definitions, it is to be feared that the results would be an important proportion of no replies and false replies and especially, which is graver, there would be the risks of creating a general mistrust

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among interrogated persons and sometimes this might even lead to an entire failure of the inquiry on hand.

We would remark, however, that in the absence of income data, the criteria used in the socio-professional classification give indirectly indications on the *average* level of income; notably the distinction between the owners and the employees, the number of workers employed by an owner or the size of the cultivated exploitation of a farmer, the seniority for an employee, the skill of a worker, etc. It has also been said that the socio-professional classification is an indirect means for classifying the persons by income group. In fact, there is not always a clear relation between the level of the income and the size of the enterprise or seniority of employee, numerous other factors intervening, but there is at least a certain correlation between the concepts. It is known that in the eyes of persons outside the groups involved, the correlation appears stronger than it is in reality, the people being to a greater extent classified by their fellow citizens in keeping with the income attributed to them rather than the income they really enjoy.

Similarly, it does not appear to be possible to use criteria of *capital property*, movable or real (dwelling houses, business, shares etc.). These criteria have a definite importance in the socio-economic situation of persons. But, likewise for the income, they cannot be obtained, especially in a census of the population.

13. On the other hand, it is certain that the extent and the nature of the *education* received exercise an important influence on the behaviour of persons. And in contrast to income, it is information which is generally set out in the census questionnaire. But the social situation of individuals is not always in keeping with their education nor with the diplomas obtained, and it is preferable to keep to the *real* socio-economic situation. Of course, this does not prevent putting the census questions on education and afterwards drawing up a table "Education × Socio-professional category", a table which will certainly not be devoid of interest.

For the same reason, it is preferable not to take into account, in order to fix the socio-professional categories, the particulars such as social environment of origin, religion, ethnical nationality, nature of the habitat (urban or rural) etc., elements whose influence on the kind of living is certain, but which should be in fact distinguished from the socio-professional category and which necessitate other tables.

14. *In conclusion*, the socio-professional classification which aims, in principle, at putting into concrete form the common concept of "social class", without however being bound to a precise theory, is carried out, in practice, by reference to characteristics easily within reach, which are essentially of occupational order, and which admittedly are linked to the previous concept.

It is from the point of view of this double consideration that it draws the name "socio-professional" used in France.

15. The operation of the coding of socio-professional categories on inquiry documents must now be indicated.

One never asks the question bluntly "To which socio-professional category do you belong?", as, in addition to the practical difficulties which this course would impose (it would be essential to incorporate in the questionnaire a complete list of the categories used together with definitions), there would be the risk that the persons involved might have a tendency to fill in the questionnaire under a higher category than the one to which they belong (or in a lower category if they feel that this would serve their interest). *The whole*

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entity of occupational information available is used through the other questions. It is therefore the Statistical Department which, when compiling the synthesis of these particulars, attributes the category code number.

For example, in the 1954 census of population, the answers to the following questions were used:

The occupation (the questionnaire asked that the maximum precision on the profession or the craft, the specialization, the grade or the employment be given).

Employment status (persons were asked whether they were owners, employees or family workers).

Industry (in addition to a direct question, the name and address of the firm had to be filled in, so as to ensure the reconciliation with the general card-index of industrial and commercial firms held by the Institut National de la Statistique et des Etudes Economiques.)

For the owners, the number of employees.

For the farmers; indicate whether owners, farmers, or sharecroppers.

For the employees: point out if one is a household worker.

For the unemployed: previous occupation.

For military recruits: civil occupation.

For retired persons: former principal occupation.*

The persons compiling the coding in the workshops of the I.N.S.E.E. used to code first of all *the occupation and the employment status*, then they had at their disposal a *correspondence table* with two main entries, indicating for each number of the occupation and of the status:

The corresponding socio-professional category, when the latter was single.

Or the various possible categories in keeping with the replies to the other questions (size of the enterprise, etc. . . .).

In other words, socio-professional classification is a *derived* classification which results from combination of the primary classifications indicated above.

16. Experience has revealed—and it is here one of the main advantages of the new classifications—that even when only a portion of the replies to the question, or replies lacking in precision, were in hand, it was often possible, nevertheless, to assign the code number of the socio-professional category or sometimes the most probable number.

For example:

The single reply "miner" to the occupation and nothing else, makes it possible to allocate the code number: Category = 65, Status = 7.†

The single reply "grocer" to the occupation makes it possible to allocate a code number: Category = 27, Status = 1 (by assuming in this case that it is a question of an independent person without any employee—the most frequent case.)

Vague replies such as "employee" or "worker" were allocated the code number: Category = 51 or 63, Status = 5 (by assuming that they concerned unskilled workers of the private sector).

So, subject to a few conventions which practically do not diminish the value of the results, *it is always possible to allocate a code number to the socio-professional category—*

* The usefulness of this question will be shown later.

† In France, the coal mines are nationalized and come under Status 7.

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when it is necessary to make provision for "not stated" items, the effects of which are sometimes important, in the other classifications (industry and occupation).

It is partly for this reason that in the last census of the population, it was not considered worth while to proceed with the automatic calculation of code numbers of the socio-professional categories by counting machines, an operation theoretically possible, inasmuch as the determination of the socio-professional category is derived from the knowledge of code numbers attributed in advance to a limited number of other criteria. Although perhaps more costly, *manual coding is better*, because it permits, every time there is a doubt, to proceed with a more complete interpretation.

17. The socio-professional categories are used, in France, not only during the census of the population, but in the majority of other censuses and investigations, and in current statistics. In the general censuses and investigations by sampling carried out by the I.N.S.E.E., or in collaboration with the latter, there is no difficulty in obtaining questionnaires incorporating all the necessary information, the investigators being systematically instructed to this effect. But it is not always the same, when the analysis of questionnaires elaborated by other services is involved, especially the forms used for the preparation of vital statistics.*

These forms are filled in by the employees of the Town Hall (Mairie) at the time of the establishment of the certificates of births, marriages, deaths, etc., and there are 38,000 Town Halls (Mairies) in France. One has to be satisfied with one question only: occupation, and to replies, often lacking in precision, to this question.

To this effect, provision has been made in the code for special numbers which do not necessitate knowledge of the size of the enterprise or the manner of developing agricultural exploitations, these are:

00. Farmers (without any other indication).
20. Owners of industry (without any other indication).
25. Merchants (without any other indication).

We thus have a classification slightly reduced which, practically, necessitates nothing more than knowledge of *strictly occupational* characteristics.

V. Classification of the Whole Population

18. So far, we have only dealt with the active population, which forms the subject of the majority of investigations. But in certain cases, it is also necessary to consider *the non-economically active population*, who represent, in bulk, half of the total population. It is necessary to provide also for subdivisions of the whole of this component; these subdivisions should be made from the same viewpoint as before, in other words, with a view to the study of social behaviour.

Experience has shown that it is useful to consider *several different modes* of classification for the non-economically active population.

(*a*) In certain cases (for example in an investigation on the listening post of the radio at various hours of the day) it is fitting to classify the non-active persons in accordance with the "proper category" which indicates their condition.†

* Also, when the work is on *archives* documents.
† Or, as already stated, the "cause of their non-activity".

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In France, the *proper categories* of non-active persons are the following*:

90. Children under 14 years old.
91. Students and pupils.
92. Military recruits.
Former active† 93. Former farmers.
94. Retired from business.
95. Retired from the public sector.
96. Former salary earners of the private sector.
97. Persons in institutions.
98. Housekeepers (unpaid).
99. Other non-active persons.

(*b*) In other cases—the most numerous—far better correlations are obtained by taking into consideration, for the children and adults at home, the *socio-professional category of the head of the household*; in fact, the socio-economic behaviour of the wives is very often determined by the social situation of their husbands.

(*c*) Finally, it is sometimes useful to consider:

For the military recruits, the socio-professional category corresponding to their civil profession.

For the former active, the socio-professional category of activity (number of code 01 to 82) corresponding to their former principal occupation; the advantage being that the categories of active persons are more detailed than the categories of former active persons.

The code used in France permits all these combinations; the choice of the system to be used is dependent upon the nature of the inquiry.

We would also state that where it has been decided to classify the dependent population according to the socio-professional category of the persons who support them, the "proper categories" can be used when, as a result of an insufficiency of replies, the profession of the supporter is not known.

19. We spoke in the preceding paragraph of the interest which often attaches to the connection of women and children with the socio-professional category of the head of the household.

This leads us to define precisely a new notion of this nature introduced recently in this kind of study, the notion of *dependence* (from the point of socio-economic view). The principal idea is to consider that the total population is divided into two large groups, the independents and the dependents.

The *independents* are, in principle, the persons who have sufficient resources to live on, the *dependents* are the persons who have poor resources or no resources at all and who depend on another person (the latter being independent) who provides for their needs; this implicitly admits that the behaviour is determined by the origin and the nature of the resources.

In fact, and notably in the census of population, no questions are set out in connection with the nature and the amount of personal resources. It is therefore necessary, failing something better, to carry out classification in an *indirect* and *conventional* manner.

* The categories whose code number is between brackets are not used in the census; they are grouped with category 99, but could be distinguished otherwise.

† For the purpose of ascertaining the *former activity* a corresponding question (mentioned above) is put in the census of the population.

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In France, the following persons were conventionally considered in the 1954 census as *independents:*

(a) *All the active persons* (whether or not they were heads of households).

(b) *All the former active persons* (whether or not they were heads of households; the majority of former owners had resources, originating from their former activity, in the shape of unearned incomes, and the majority of the former salary earners had retirement pensions).

(c) *The other heads of households;* that is, heads of households who were neither active nor formerly active, and for whom it is admitted that their independent character stems from their position, in fact, as heads of households.

and, as *dependents:*

All persons who were not active, or formerly active or heads of households.

20. It is clear that this method, the only one possible, is far from being perfect; numerous cases can be imagined in which the rule applied leads to something meaningless. For example: a former owner having become bankrupt can draw no profit whatever from his former activity; a wife having a personal fortune which yields a higher revenue than the remuneration of her husband, supposed to be active, will be considered as dependent on her husband and classified in the socio-professional category of the latter, etc. . . . Objections could also be raised in cases where the children of couples in which both husband and wife work and therefore both contribute to the household expenditure, are systematically all linked up to the socio-professional category of their father.

Finally—and it is perhaps here that the major inconvenience of the system prevails—in cases where several generations of adults are incorporated in a single household, the person declared as the head of the household in the census, out of respect, or because he is the occupier of the dwelling in which the household lives, is often an aged person no longer working, whilst the principal "breadwinner" or "breadwinners" of the household (that is to say the persons who bring in most of the money and often have the most influence in the household) are the adults of the second generation; and yet the various persons of the household are linked to the category of the head of the household, that is to say that of the aged person. Let us take for example a retired old man who lives with his son who is a teacher, the wife of the latter, not working, and the son of the young couple. The wife of the teacher and her son, both dependent, will be linked to the socio-professional category of the grandfather (that is to say to the category of the "retired"), whereas they depend in fact upon the teacher.

All these examples of the defects of the procedure used—the importance of which should not be exaggerated—will necessitate in the next census a more detailed analysis of the structure of households to ensure that the real links in the structure of households will be better determined.*

21. Finally, by proceeding in this manner, we finish by classifying the whole population in socio-professional categories, it being understood that the *independent* persons will be classified in their proper category and the *dependent* persons in the category of their presumed *breadwinner*.

* We would point out that heretofore the procedure followed was briefer by linking all the members of a household, whether they work or not, to the category of the head of the household; therefore, in the last example which we have just cited, the teacher also would have been linked to the category of retired persons.

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This helps to provide a reply, at least approximately, to questions like this: How many persons are *supported by* farmers, owners, the employees, the workers, government officials, etc.

Here we would add that a similar question is often put to the statistician: How many persons are supported by a particular industry or trade, by agriculture or mines? The procedure followed is similar to the one just described, i.e. linking each dependent person to the *branch of industry* of his supporting person.

VI. Practical Uses of Socio-professional Classifications

22. We are now going to outline precisely the method of using the socio-professional classification.

We would remark, first of all, that the socio-professional classification *does not serve as a substitute* for the traditional professional classifications: occupation, industry and status. It *superposes* the latter.

The traditional classifications ensure, separately or by combining several amongst them, a detailed and precise description of occupational characteristics; each one may be extended according to need, they are irreplaceable in the uses for which they were devised: structure of the economy, structure of occupational groups, etc.

A first possible use of the socio-professional classification is the *general and synthetic description* of the occupational situation of persons, this being due to the manner in which the classification is operated. But this does not constitute the principal use of the classification. As a matter of fact, it is more often required for the study of *correlations* between, on the one hand, the occupational characteristics of individuals or of families, and, on the other hand, other numerous and very diverse criteria such as housing comfort, infantile mortality, radio listening-in, reading, the distribution of tuberculosis, religious practice etc.

For this object the traditional occupational classifications *cannot be used*, because:

(*a*) The classifications by individual occupation or by industry currently used include considerably large numbers of items* so that tables which would be obtained by using these classifications would be elaborate and unusable on account of their volume.

(*b*) The use of these same classifications reduced to a small number of items, or the use of employment status which, by itself only consists of a few items, permits one to obtain tables easy to handle, but these would not be interesting as each of the groups retained would then be very heterogeneous.

On the contrary, the socio-professional classification, which consists of an acceptable number of relatively homogeneous groups, is particularly suitable for such cross tabulations. It has proved to be particularly suitable in certain inquiries involving a restricted number of persons, especially in inquiries *by sampling*, for summarizing into a small number of items the occupational characteristics of individuals or of families, for whom it was evidently not possible to use the traditional occupational classification, too detailed—unless one agreed to accept statistical tables, a great portion of which would be empty.

The socio-professional classification has proved equally efficacious for the *preliminary stratification* of the population for inquiries by sampling, market surveys, etc. Furthermore it ensures the easy classification of the *whole of the population*, including persons not

* The nomenclatures employed in France each consist of about a thousand items.

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economically active and dependent persons, and it is often indispensable to be able to classify such persons when they constitute an important part of the population studied (for example, the study of leisure time, clothing, etc.).

Finally, as mentioned above, it is not indispensable, in order to allocate the code numbers of the socio-professional categories, to dispose of all the details which would be sometimes necessary for allocating the code numbers of other occupational characteristics; it is possible, sometimes, to allow slight sacrifices in the exactitude of classification of persons giving incomplete information.

VII. British Classifications

23. So far, reference has only been made to the efforts made in France to reach a satisfactory classification of the population, in accordance with its principal socio-economic characteristics. Several other countries have undertaken similar researches. In particular, I do not ignore the extremely interesting work carried out in this direction in Great Britain on the occasion of the census of the population, but other persons are infinitely more qualified than myself to speak on that subject. I am, however, going to outline what I know of these works, so as to bring them into relation to the French trials about which I have spoken, and we will see that, though they follow a different approach, they finally end in the formation of categories similar in many respects. I apologize in advance if I make a few mistakes of interpretation as regards the British classifications.*

In the British 1951 census, in addition to the traditional classifications of industry and of occupation, "status", "social class", "socio-economic group" and "method of remuneration" were put in bold relief.

24. "*Status*" includes the following categories and applies to persons 15 years old and over:

(*a*) Employers.
(*b*) Workers on own account.
(*c*) Managers.
- c1. Chairman of Company, Managing directors, Directors, General superintendent of large undertakings.
- c2. Manager of primary operations in manufacturing or other business.
- c3. Manager of office or subsidiary department.

(*d*) Employees (other than classified elsewhere).
(*e*) Apprentices, articled pupils, etc.
(*f*) Part time workers.
(*g*) Unpaid family workers.
(*h*) Out of work.
(*j*) Retired.
(*k*) Persons to whom the classification does not apply (notably students, adults at home, persons living on an unearned income).

25. "Social class", "socio-economic group" and "method of remuneration" are three complementary classifications each obtained by grouping entire categories of occupations in a different manner; they are therefore classifications that stem from occupation.

The social class is a grouping of occupations in categories and has been used in Great Britain since 1911; this concept is that of the rank occupied in esteem within the community by the occupation.

* Documentation: *Document Conf. Eur. Stats.*/WG6/EG 20 of the 21st January, 1956, published by the Statistical Commission and Economic Commission for Europe. Conference of European statisticians.

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The classes are as follows (the detail is more or less elaborated in accordance with the purpose of the inquiries):

I. Professional etc. occupations.
II. Intermediate occupations.
III. Skilled occupations.
  (*a*) Mine workers.
  (*b*) Transport workers.
  (*c*) Clerical workers.
  (*d*) Armed Forces.
  (*e*) Others.
IV. Partly-skilled occupations.
  (*a*) Agricultural workers.
  (*b*) Others.
V. Unskilled occupations.
  (*a*) Building and dock labourers.
  (*b*) Others.

The *socio-economic groups* allow more elaborate analysis for certain researches, they include:

A. Agricultural:
  1. Farmers.
  2. Agricultural workers.
B. Non-agricultural:
  I. Non-manual workers—
    3. Higher administrative professional and managerial.
    4. Other administrative, professional and managerial.
    5. Shopkeepers (including proprietors and managers of wholesale businesses).
    6. Clerical workers.
    7. Shop assistants.
    8. Personal service.
  II. Manual workers—
    9. Foremen.
    10. Skilled workers.
    11. Semi-skilled workers.
    12. Unskilled workers.
C. (Special category):
  13. Armed Forces (other ranks).

The method of remuneration concerns only the employees and corresponds to the method of payment of the remuneration most generally used in the occupation (wages, salary, etc. . . .).

Of these three classifications, it is clear that the *socio-economic groups* are the most similar to the French socio-professional categories. Without being entirely comparable, these two classifications can, however, be related—the comparison of tabulations being rather difficult, on account of the inevitable differences in the definitions, especially as regards the marginal groups.

VIII. Proposed International Recommendation

26. In numerous countries, other than Great Britain and France, the statisticians have tried for some time to put into practice a new style of classification involving more especially the active population and superposing other factors upon the traditional occupational classification. These trials have given rise to evidently different classifications from one country to another, but all approaching the same objective, viz., a stratification of the population in relation to the economic and social behaviour of individuals.

It is remarkable that the various experiments carried out in countries with *different types of economy* reveal *numerous common points* as regards the principles which have

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governed the development of the classifications, the methods adopted and the constituted groups.

This shows the existence of a common need and, in the Conference of European Statisticians, the "Working group on censuses of population and housing" was entrusted with the elaboration of an international recommendation suitable for Europe which I shall outline briefly.

27. In this proposal, the whole population is first of all subdivided according to *the type of activity:*

I. Economically active population:
   (*a*) Persons having an employment.
   (*b*) Unemployed (optional subdivision).
II. Non-economically active population:
   (*a*) Independent persons drawing or presumed to be drawing their means of existence principally from private resources.
   (*b*) Persons depending on others.

The "independent" persons (I and II*a*) are then classified according to *status.*

Basic Categories	*Optional Categories*
(*a*) Employers.	—
(*b*) Workers on own account.	—
(*c*) Employees.	Apprentices.
(*d*) Family workers.	Paid family workers. Unpaid family workers.
(*e*) Members of producers' co-operatives.	—
(*f*) Economically active persons who cannot be classified according to status.	—
(*g*) Independent inactive persons.*	

Finally, the population is divided into *socio-economic groups.* The classification provides categories for the economically non-active persons who are dependents, but it is understood that, in accordance with circumstances, the dependent persons can be classified in the socio-economic groups of persons on whom they depend. The list of socio-economic groups is as follows:

Basic Categories	*Useful Subdivisions*
A. *Economically Active Population*	
1. Farmers who are employers.	(*a*) Large farms. (*b*) Small farms.
2. Farmers on own account without employees.	
3. Members of agricultural producers' co-operatives.	
4. Agricultural workers.	
5. Employers in industry and commerce—large enterprises.	(*a*) Industry. (*b*) Commerce.
6. Employers in industry and commerce—small enterprises.	(*a*) Industry. (*b*) Commerce.
7. Owners of establishments in industry and of commerce—own account workers without employees.	(*a*) Industry. (*b*) Commerce.
8. Liberal and related professions.	
9. Members of non-agricultural producers' co-operatives.	
10. Managers of enterprises and companies.	
11. Senior employees (non-manual).	
12. Intermediate employees, junior employees and sales workers (non-manual).	(*a*) Intermediate employees. (*b*) Junior employees and salesmen.

* To be used when it is desired to classify the dependent persons in accordance with the status of persons who support them.

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Basic Categories	*Useful Subdivisions*
A. *Economically Active Population (contd.)*	
13. Foremen, skilled, semi-skilled and specialised workers.	(*a*) Foremen. (*b*) Skilled, semi-skilled and specialized workers.
14. Labourers.	
15. Personal service (domestic servants, cleaners, door keepers), and related workers.	
16.*Conscripts.	
17. Economically active persons who could not be classified in the above-mentioned categories.	
B. *Economically Non-active Population*	
18. Former farmer-employers.	
19. Former employers who were not farmers.	
20. Former employees.	(*a*) Former agricultural workers. (*b*) Former non-manual employees. (*c*) Former manual workers and personal service.
21. Other non-economically independent persons.	(*a*) Students. (*b*) Pensioners. (*c*) Others.
22.†Children below minimum school leaving age.	
23.†Students, pupils above minimum school leaving age.	
24.†Housewives.	(*a*) Farmers; households. (*b*) Other households.
25.†Other adults at home.	
26.‡Inmates of institutions.	

Certain countries have pointed out that it is not always possible to obtain the necessary detailed information for the classification of persons in the above-mentioned categories, especially in current statistics (vital statistics, etc.); it has been admitted that, in case of need, it would be possible to carry out the under-mentioned re-grouping:

Summary Groups	*Basic Categories*
Farmers	1 and 2
Members of agricultural producer co-operatives	3
Agricultural workers	4
Employers and own account workers in industry and commerce	5, 6 and 7
Liberal and related professions	8
Members of non-agricultural producer co-operatives	9
Managers of enterprises	10
Employees—non-manual	11 and 12
Employees—manual	13 and 14
Personal service	15
Conscripts	16
Economically active persons who could not be classified in the above categories	17
Economically non-active independent population	18 to 21
(Not used)	22 to 26

IX. Examples of the Use in France of Socio-professional Categories

28. To end, we are going to give a few examples of results of various inquiries carried out in France, in which the socio-professional categories, cross tabulated with diverse factors, have revealed interesting correlations.

* Category used when military service is a legal requirement and consequently these persons are not allocated the number of the category corresponding to their situation in civil life but are considered as a special group.

† Categories used when dependent persons are not linked to the category of persons on whom they depend, but are characterized by their own proper situation.

‡ Category used when persons in institutions are not classified in their proper category but are considered as a special group.

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(a) *Weight and height of scholars.*—An investigation by sampling was made of the weight and the height of scholars. These different measurements were tabulated for the different socio-professional categories of the parents.

Here are the average heights and average weights for three ages:

Age of Scholars in Years	*Socio-professional Group of the Head of the Household*						
	*Farmers**	*Agricultural Workers*	*Employers and Own Account†*	*Liberal Professions and Senior Non-manual Workers*	*Intermediate Non-manual Workers*	*Other Non-manual Workers*	*Manual Workers*
			A—Boys				
			Average Height (in cm.)				
5½	110·9	109·5	111·1	113·1	110·6	110·3	110·3
8½	127·3	125·8	128·3	128·9	128·4	128·4	126·6
11½	141·1	140·7	141·6	143·3	142·5	142·5	141·2
			Average Weight (in kg.)				
5½	18·8	18·4	19·0	19·7	18·6	18·2	18·5
8½	25·6	24·7	26·2	26·3	25·8	25·9	25·2
11½	33·6	33·5	34·5	35·9	34·8	34·5	33·7
			B—Girls				
			Average Height (in cm.)				
5½	110·3	109·0	110·4	112·0	110·4	110·2	110·2
8½	126·4	125·2	126·9	128·4	127·5	126·3	126·1
11½	142·6	142·2	143·4	145·4	144·0	144·7	142·4
			Average Weight (in kg.)				
5½	18·5	17·9	18·9	19·4	18·6	18·4	18·4
8½	24·7	24·3	25·2	26·1	25·7	25·2	24·8
11½	34·6	34·1	35·3	36·7	36·1	35·6	34·7

* Essentially employers.
† Essentially artisans and small merchants.

The complete table showing the deficiency in average height and weight of the sons of manual workers and agricultural workers, as compared with the sons of liberal professions and senior administrative staff, is given below.

Age of Scholars in Years	*Defect of Height and Weight (Boys) According to the Socio-professional Category of the Father*			
	Manual Workers (Non-agricultural)		*Agricultural Workers*	
	Height (cm.)	*Weight (kg.)*	*Height (cm.)*	*Weight (kg.)*
5½	2·8	1·2	3·6	1·3
6	2·9	1·2	3·1	1·3
6½	2·2	1·1	2·6	1·5
7	2·7	1·5	2·9	1·7
7½	3·0	1·5	3·1	1·8
8	2·2	1·3	2·8	1·7
8½	2·3	1·1	3·1	1·6
9	2·4	1·4	3·6	1·8
9½	2·1	1·3	3·7	2·0
10	1·9	1·3	3·5	2·1
10½	2·1	1·6	3·3	2·4
11	2·0	2·0	3·1	2·7
11½	2·0	2·2	2·5	2·4

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What the scholars drink.—On the occasion of the same inquiry, a question was put about the drink usually consumed at the mid-day meal by scholars. The results related to the socio-professional category of the parents were:

Socio-professional Group of the Head of the Household	*Percentage of Scholars who Drink Nothing but Water at the Mid-day Meal*
Liberal professions and senior personnel	29·1
Intermediate personnel	19·6
Other non-manual workers	17·0
Manual workers	16·1
Employers in industry and commerce	15·7
Farmers	11·6
Agricultural workers	10·2

(b) *Inquiry on employment.*—On the occasion of this periodical inquiry, essentially intended for ascertaining the level of the employment, questions are put on various subjects. For example, the average number of sick leave days taken during the year 1951 is shown below:

Socio-professional Category	*Average Number of Sick Leave Days*	
	Men	*Women*
Farmers	7·4	6
Artisans and industrial employers	10	7
Merchants	6	6
Liberal professions and clergy	5	—
Literary and scientific professions	1·5	6·5
Technicians	6	—
Other senior personnel	5	—
Intermediate personnel of public sector	10·5	13
Other non-manual workers	5	9
Army and police	13·5	—
Intermediate personnel of private sector, foremen	9	—
Apprentices	5	1
Manual workers (non-agricultural) (skilled)	10·5	14
Miners	19	—
Unskilled workers (non-agricultural)	14·5	16
Agricultural workers	6	7
Servants	—	3
Housekeepers	—	7
Other personal service	11	8
Total	8·6	7·9

The average duration of occupation in the establishment, at the time of the inquiry, of persons interrogated, is given below:

Socio-professional Category	*Average Duration (in Years)*	
	Men	*Women*
Farmers	18·3	16·7
Small employers	14·2	13·5
Liberal and intellectual professions	14·7	12·0
Non-manual workers	13·3	8·5
Manual workers (non-agricultural)	9·1	7·5
Agricultural workers	5·9	8·2
Other workers	9·3	7·4
Total	12·5	11·6

It will be noticed, in previous examples, that *different re-groupings of the basic list of socio-professional categories were used in accordance with circumstances*; it will also be observed in the examples which follow that sometimes *a selection of a few categories is carried out from the basic list.*

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The following table relates to the level of education of wives, in the households interrogated in the same inquiry, for each socio-professional category of the husband.

With a view to ensuring a certain degree of homogeneity, this classification has been limited to women under 40 years of age

Socio-professional Category of the Husband	*Age of the Wife at the Time of Termination of Studies (Percentage)*					
	Less than 13 Years	*13–14 Years*	*15–17 Years*	*18 Years*	*N.D.*	*Total*
Farmers	20	72	5	1	2	100
Agricultural workers	24	68	4	1	3	100
Small employers	10	60	18	5	7	100
Liberal professions	3	17	20	40	20	100
Intellectual professions	7	24	35	26	8	100
Intermediate personnel	7	58	18	11	6	100
Other non-manual workers	6	59	22	6	7	100
Manual workers	15	70	8	2	5	100
Total	13	63	13	5	6	100

Here is a table illustrating social mobility in France. It is a cross tabulation indicating, in a proportion of 1,000, the category of persons for each category of the father (table limited to men 40 to 59 years old)

Socio-professional Category of Persons	*Socio-professional Category of the Father at the Time when the Person left School*					
	Farmers	*Small Employers*	*Senior Personnel*	*Clerks**	*Workmen (Non-agricultural)*	*Agricultural Workmen*
Farmers	597	40	33	41	31	135
Small employers	78	477	124	115	116	127
Senior personnel	17	62	512	183	20	4
Clerks	72	147	215	317	194	77
Workmen (non-agricultural)	176	259	107	344	618	400
Agricultural workmen	60	15	9	—	21	257
Total	1,000	1,000	1,000	1,000	1,000	1,000

* Intermediate administrative staff, employees, etc.

(c) *Listening-in to the radio* —The object of this inquiry was to ascertain the habits and tastes of French listeners. Here are a few tables extracted from this inquiry

Socio-professional Category	*Periods of Listening in the Day (Percentage of Hearers who Listen)*				*Time at which Listening Ends (Percentage of Hearers who Cease to Listen)*			
	Before Noon	*Between Noon and 14 h.*	*Between 14 and 18 h.*	*After 18 h.*	*Before 21 h.*	*From 21 to 22 h.*	*From 22 to 23 h.*	*After 23 h.*
Farmers	48	75	8	93	19	47	30	4
Artisans	45	73	17	92	21	31	38	10
Small merchants	43	70	16	88	14	28	45	13
Large employers / Senior personnel	43	69	15	90	11	18	52	19
Of whom intellectual	47	69	16	90	9	17	57	17
Intermediate personnel	47	65	7	97	10	21	51	18
Non-manual workers	48	59	11	98	7	20	59	14
Skilled manual workers	39	60	15	96	10	39	43	8
Unskilled workers	37	67	14	94	20	43	32	5
Retired	55	75	27	97	8	32	51	9
Other aged and non-active	40	68	25	97	34	26	34	6
Housekeepers	61	79	39	80	14	36	41	9
Total	48	70	22	93	15	33	42	10

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Utilization of Short Waves—Correspondence with the Radio Administration—Choice of Programmes

Socio-professional Category	*Percentage of Hearers Who Use the Short Waves*	*Percentage of Hearers Having Declared They have Never Written to the Radio, but Have Had the Wish to Do So*	*Percentage of Listeners Having Asked for an Increase in the Broadcast of Symphony Music*
Farmers	16	10	5
Artisans	36	14	14
Small merchants	24	19	10
Large employers and senior personnel	44	23	20
Of whom intellectual	46	27	21
Intermediate personnel	35	28	21
Non-manual workers	28	24	12
Skilled manual workers	30	20	8
Unskilled workers	17	19	6
Retired	31	16	19
Other aged and non-active	16	11	5
Housekeepers	26	18	8
Total	27	18	10

(d) *General mortality and infantile mortality*—The sorting out of statistics of the register office permits the calculation of the rates of mortality for socio-professional categories.*

The mortality rates, (comparative rates) for a few causes of deaths (men between 25 and 54 years old—year 1955) are given below:

Socio-professional Category	*Rate of Mortality per* 10,000			
	Cancer and Leukaemia	*Cirrhosis of the Liver*	*Accidents*	*Suicides*
Intermediate personnel	5·4	1·3	4·4	1·5
Liberal professions and senior personnel	6·2	1·0	5·4	1·2
Liberal professions	7·0	1·4	5·6	1·8
Farmers	6·8	2·8	5·2	3·2
Intermediate personnel and non-manual workers	7·7	2·5	6·4	1·8
Industrials and artisans	7·6	4·0	6·0	1·8
Farmers and agricultural workers	6·7	2·5	7·3	4·0
Employers in industry and commerce	9·0	4·5	6·6	2·4
Non-manual workers	9·5	3·4	7·9	2·0
Agricultural workers	6·2	1·8	12·1	5·6
Manual workers	8·4	3·4	12·1	2·9
Merchants	10·3	5·0	7·1	3·0
Miners	6·9	2·2	13·3	2·0
Unskilled workers (non-agricultural)	9·3	4·5	18·0	4·8
Sailors and fishermen	10·8	4·0	23·8	2·5

The following table shows the rate of infantile mortality in accordance with the socio-professional category of the father (legitimate children of the two sexes, number of deaths per 1,000 living births)

* With regard to general mortality, there is a certain lack of precision due to the fact that different documents are involved (death registrations and census schedules) on which the socio-professional category declared for the same person can be different; with regard to infantile mortality, on the contrary, the category of the father declared at the time of birth is taken for the children who are dead, thus removing this source of discrepancy.

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Socio-professional Category of the Father	*Rate of Infantile Mortality per 1,000 (Age)*		
	0–12 *Months*	*Less than 1 Month*	1 *to* 12 *Months*
Liberal professions	19·1	10·5	8·6
Engineers	21·7	12·2	9·5
Employers in industry and senior personnel	23·9	12·9	11·0
Intellectual professions	24·7	13·8	10·9
Technicians	25·7	12·4	13·3
Sales workers	28·2	13·6	14·6
Non-manual workers	30·5	14·7	15·8
Intermediate personnel	32·4	14·9	17·5
Merchants	34·5	17·6	16·9
Army and police	35·2	17·3	17·9
Artisans	36·4	19·2	17·2
Skilled manual workers	42·5	18·6	23·9
Total	43·1	19·0	24·1

(e) *Household equipment—Clothing*—A household inquiry by sampling has made it possible to ascertain the equipment in the households for each socio-professional category of the head of the household

Socio-professional Category of the Head of the Household	*Number of Apparatuses Installed in 1954 per 1,000 Lodgings*					
	Refrigerators					
	Gas or Butane	*Electricity*	*Vacuum Cleaners*	*Polishers*	*Washing Machines*	*Radio Sets*
Farmers	2	22	18	3	73	595
Agricultural workers	—	5	10	—	18	522
Employers	21	159	270	57	132	829
Liberal professions, senior personnel	50	378	705	202	234	948
Intermediate personnel	12	143	410	59	164	896
Non-manual workers	10	89	188	24	67	845
Manual workers	4	29	63	4	85	785
Non-active	2	35	104	14	38	561
Total	8	67	140	24	84	717

Another inquiry by sampling has given the following results for the average expenditure of households for clothing in 1953

Socio-professional Category	*Average Expenditure per Household (Francs)*	*Average Number of Persons*	*Average Number of Children*
Farmers	65,640	3·9	0·8
Artisans	74,960	3·2	0·6
Small merchants	84,240	3·0	0·6
Industrials	176,000	3·4	0·7
Large merchants	141,640	3·5	0·7
Liberal professions	208,600	3·4	0·9
Clergy	37,480	1·7	—
Engineers	203,120	4·1	0·9
Other senior personnel	190,400	3·4	0·9
Intermediate personnel	124,520	3·1	0·7
Employees	88,920	3·0	0·6
Foremen	110,160	3·3	0·7
Unskilled workers	58,560	3·2	0·9
Other workmen	73,280	3·4	0·9
Miners	103,040	4·1	1·4
Agricultural workers	48,080	3·3	1·0
Servants	47,280	2·2	0·4
Housekeepers	18,960	1·5	0·1
Guardians	55,800	2·3	0·3
Other personal service	68,800	2·6	0·6
Army and police	99,720	3·6	1·2
Non-active	29,200	1·9	0·1
Total	73,080	3·1	0·6

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(f) *Lodging* —Here are the results of an inquiry of opinions about the lodging of French people

Socio-professional Category	*Percentage of Persons Who*			
	Consider Themselves Badly Lodged		*Consider Themselves Well Lodged*	
	And Wish to Change Their Lodgings	*But Do Not Wish to Change Their Lodgings*	*But Wish to Change Their Lodgings*	*And Do Not Want to Change Their Lodgings*
Artisans	18	10	6	66
Small merchants	16	16	5	63
Liberal professions and senior personnel	17	5	8	70
Intermediate personnel	27	6	10	57
Non-manual workers	32	5	6	57
Manual workers	34	7	5	54
Non-active	8	10	2	80
Others	22	7	5	66
Total	22	8	5	65

29. We could cite other examples of results obtained in France on various subjects (saving, possession of telephone, divorces, size of households, number of children, religious belief, study of intermediate personnel*, owners of cars, salaries and income, household budgets etc.). But we think that the preceding list, already long, is sufficient to show the use of the socio-professional classification applied to the most varied demographic and economic subjects.

X. Appendices

To conclude we give:

In Appendix I, bibliographic references indicating especially the source of the principal examples cited above.

In Appendix II, the division in France, in the 1954 census, of the population by socio-professional category, and the division of households, by the socio-professional category of the head of the household.

In Appendix III, the contents of the categories of the socio-professional classification.

APPENDIX I

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APPENDIX II

Classification of the Whole French Population into Socio-professional Categories
1954 *Census*

Socio-professional Category	*Code*	*Population Classified According to Proper Category*		*Population Classified According to Proper Category (Independents) or to Category of Head of Household (Dependents)*	
		Males	*Females*	*Males*	*Females*
FARMERS	0				
Without any other indication	00	91,900	53,120	122,660	121,780
Owners	01	1,460,980	998,200	1,909,460	1,971,060
Tenants	02	660,720	509,160	965,100	962,520
Sharecroppers	03	127,580	82,180	176,280	170,760
Total		2,341,180	1,642,660	3,173,500	3,226,120
AGRICULTURAL WORKERS	1	980,540	170,980	1,298,160	844,180
EMPLOYERS IN INDUSTRY AND COMMERCE	2				
Industrials	21	73,880	11,520	108,960	104,960
Artisans	22	606,020	128,260	839,200	776,960
Fishermen	23	21,120	3,180	30,360	24,620
Large Merchants	26	130,880	52,240	185,220	187,080
Small merchants	27	608,380	660,360	867,440	1,207,440
Total		1,440,280	855,560	2,031,180	2,301,060

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APPENDIX II—(*cont.*)

Socio-professional Category	*Code*	*Population Classified According to Proper Category*		*Population Classified According to Proper Category (Independents) or to Category of Head of Household (Dependents)*	
		Males	*Females*	*Males*	*Females*
LIBERAL PROFESSIONS AND SENIOR PERSONNEL	3				
Liberal professions	30	102,840	17,080	163,500	155,880
Literary and scientific professions	32	49,100	32,060	76,720	86,180
Engineers (private sector)	33	79,420	1,720	123,160	105,800
Senior administrative personnel, (private sector)	34	105,280	10,720	165,080	149,120
Senior personnel (public sector)	35	143,940	12,080	231,200	205,440
Total		480,580	73,660	759,660	702,420
INTERMEDIATE PERSONNEL	4				
Teachers. Medical and social service (private sector)	41	34,860	88,840	50,520	121,520
Teachers. Medical and social service (public sector)	42	92,340	183,400	149,320	279,020
Technicians (private sector)	43	176,020	17,200	243,360	174,120
Intermediate administrative personnel (private sector)	44	259,380	75,780	366,040	339,000
Intermediate personnel (public sector)	45	148,840	62,880	219,800	228,380
Total		711,440	428,100	1,029,040	1,142,040
NON-MANUAL WORKERS	5				
Office employees (private sector)	51	314,360	536,600	430,340	804,240
Office employees (public sector)	42	454,860	331,960	675,040	820,000
Sales workers	53	217,040	223,660	290,200	386,640
Total		986,260	1,092,220	1,395,580	2,010,880
MANUAL WORKERS	6				
Foremen (private sector)	60	131,860	9,620	191,600	170,340
Skilled workmen (private sector)	61	1,907,580	582,260	2,641,020	2,170,540
Skilled workers and foremen (public sector)	62	342,480	23,980	519,820	453,000
Semi-skilled workers (private sector)	63	1,037,720	534,540	1,512,260	1,486,160
Semi-skilled workers (public sector)	64	252,760	34,120	395,960	342,960
Miners	65	235,460	1,860	375,140	308,660
Sailors and fishermen	66	50,080	1,100	69,040	49,680
Apprentice workers	67	165,360	43,680	165,640	44,500
Unskilled workers	68	866,140	244,500	1,212,280	969,480
Total		4,989,440	1,475,660	7,082,760	5,995,320
PERSONAL SERVICE	7				
Servants (resident)	70	11,640	319,060	17,540	326,680
Housekeepers (paid, non-resident)	71	—	213,340	13,300	233,540
Other personal service	72	185,860	253,880	253,440	403,000
Total		197,500	786,280	284,280	963,220
OTHER CATEGORIES	8				
Artists including musicians	80	29,440	16,280	37,460	36,300
Clergy	81	54,360	100,340	57,280	111,720
Army and police	82	294,060	4,560	444,460	330,980
Total		377,860	121,180	539,200	479,000
TOTAL 0 TO 8		12,505,080	6,646,300	17,593,300	17,664,240

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APPENDIX II—(*cont.*)

Socio-professional Category	*Code*	*Population Classified According to Proper Category*		*Population Classified According to Proper Category (Independents) or to Category of Head of Household (Dependents)*	
		Males	*Females*	*Males*	*Females*
Students and pupils of 14 years and more	91	694,720	672,540	21,300	12,640
Military recruits	92	200,420	—	200,420	—
Former active: Former farmers	93	322,640	416,540	349,860	541,780
Former active: Retired from business	94	162,080	241,940	181,120	346,180
Former active: Retired from the public sector	95	584,260	176,180	656,380	661,400
Former active: Former employees of the private sector	96	487,360	640,820	545,840	940,440
Total of former active		1,556,340	1,475,480	1,733,200	2,489,800
Other non-active persons	99	5,553,920	13,458,480	503,000	1,646,340
TOTAL OF 9		8,005,400	15,606,500	2,457,920	4,148,780
Unclassified		—	—	459,200	439,780
TOTAL FRANCE		20,510,480	22,252,800	20,510,480	22,252,800

Classification of French Households According to the Socio-professional Category of the Head of Household

1954 *Census*

Socio-professional Category of the Head of Household	*Code*	*Households According to the Sex of the Head*	
		Males	*Females*
FARMERS—			
Without any other indication	00	60,680	9,340
Owners	01	978,500	147,000
Tenants	02	451,100	34,340
Sharecroppers	03	76,300	4,140
Total	0	1,566,580	194,820
AGRICULTURAL WORKERS	1	454,020	28,440
EMPLOYERS IN INDUSTRY AND COMMERCE—			
Industrials	21	66,640	4,160
Artisans	22	512,400	39,180
Fishermen	23	15,920	280
Large merchants	26	113,680	10,540
Small merchants	27	533,620	164,380
Total	2	1,242,260	218,540
LIBERAL PROFESSIONS AND SENIOR PERSONNEL—			
Liberal professions	40	95,920	3,960
Literary and scientific professions	32	38,660	10,200
Engineers (private sector)	33	68,120	540
Senior administrative personnel (private sector)	34	96,380	4,700
Senior personnel (public sector)	35	129,620	4,820
Total	3	428,700	24,220

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APPENDIX II

Classification of French Households According to the Socio-professional Category of the Head of Household—(contd.)

Socio-professional Category of the Head of Household	*Code*	*Households According to the Sex of the Head*	
		Males	*Females*
INTERMEDIATE PERSONNEL—			
Teachers. Medical and social service (private sector)	41	24,600	23,880
Teachers. Medical and social service (public sector)	42	78,820	44,120
Technicians (private sector)	43	131,120	4,140
Intermediate administrative personnel (private sector)	44	214,020	21,960
Intermediate personnel (public sector)	45	127,040	18,620
Total	4	575,600	112,720
NON-MANUAL WORKERS—			
Office employees (private sector)	51	224,940	103,120
Office employees (public sector)	52	368,420	74,360
Sales workers	53	139,240	44,920
Total	5	732,600	222,400
MANUAL WORKERS—			
Foremen (private sector)	60	121,880	3,120
Skilled workers (private sector)	61	1,246,900	128,960
Skilled workers and foremen (public sector)	62	278,100	8,240
Semi-skilled workers (private sector)	63	740,100	117,860
Semi-skilled workers (public sector)	64	211,960	8,720
Miners	65	168,280	720
Sailors and fishermen	66	30,260	220
Apprentice workers	67	1,400	80
Unskilled workers	68	533,480	74,700
Total	6	3,332,360	342,620
PERSONAL SERVICE—			
Servants (resident)	70	4,260	24,440
Housekeepers (paid, non-resident)	71	—	91,320
Other personal service	72	141,860	76,120
Total	7	146,120	191,880
OTHER CATEGORIES—			
Artists including musicians	80	22,060	4,860
Clergy	81	29,340	740
Army and police	82	211,080	1,080
Total	8	262,480	6,680
TOTAL 0 TO 8		8,740,720	1,342,320
Students and pupils of 14 years and more	91	17,460	5,700
Military recruits	92	—	—
Former active: Former farmers	93	215,220	178,820
Former active: Retired from business	94	134,720	118,000
Former active: Retired from the public sector	95	536,380	91,420
Former active: Former employees of the private sector	96	402,260	326,460
Total of former active		1,288,580	714,700
Other non-active persons	99	224,480	1,084,080
TOTAL 9		1,530,520	1,804,480
TOTAL FRANCE		10,271,240	3,146,800

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Appendix III

Socio-professional Categories and Status (Contents of the Categories)

Socio-professional Categories

Code No.	*Designation*	*Comments*
0–	Farmers—	
00	Farmers, without other details	Farmers who cannot be classed as owners, tenants or sharecroppers.
01	Owners.	
02	Tenants.	
03	Sharecroppers.	
1–10	Agricultural Workers.	
2–	Employers in Industry and Commerce—	
21	Industrials	Employing more than 5 employees.
22	Artisans	Working independently, or employing up to 5 workers.
23	Fishermen	Employed fishermen are classed under 66.
26	Large merchants	Employers with over 2 employees. In addition, wholesalers and certain other tradesmen are automatically classed under 26 (e.g. jewellers).
27	Small merchants	Working independently, or employing up to 2 employees.
3–	Liberal Professions and Senior Personnel*—	
30	Liberal professions	This category is fairly narrowly interpreted, with employees included only exceptionally.
32	Literary and scientific professions	Usually include occupations regarded as being intellectual (e.g. professor, college headmaster, geographer, writer, librarian, salaried physician, salaried architect, etc. . . .).
33	Engineers (private sector)	Includes all high-grade technicians working in non-public concerns.
34	Senior administrative personnel (private sector)	All persons occupying managerial posts (other than technical ones).
35	Senior personnel (public sector)	Including in the army: officers; in public services: senior officers.
4–	Intermediate Personnel†—	
41	Teachers. Medical and social service (private sector)	Intermediate category including e.g. school teacher, music teacher, midwife, graduate nurse, reporter, proof corrector, mapper etc. . . .
42	Teachers. Medical and social services (public sector)	Similar to the above, but in public establishments.
43	Technicians (private sector)	Category lying between that of engineers (No. 33) and that of wage-earners (No. 6).
44	Intermediate administrative personnel (private sector)	Category similar to the preceding one, including e.g. management secretary, bookkeeper (salaried), notary's head clerk, insurance broker, travelling salesman, etc. . . .
45	Intermediate personnel (public sector)	Category lying between that of senior official (No. 35) and that of clerk (No. 52).
5–	Non-manual Workers—	
51	Office employees (private sector).	
52	Office employees (public sector).	
53	Sales workers.	

* These groups generally include employees and sometimes independent workers or employers.
† The groups generally include employees and sometimes independent workers or employers.

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APPENDIX III

Socio-professional Categories and Status (Contents of the Categories)—(contd.)

Socio-professional Categories

Code No.	*Designation*	*Comments*
6–	MANUAL WORKERS—	
60	Foremen (private sector).	
61	Skilled workers (private sector).	
62	Skilled workers and foremen (public sector).	
63	Semi-skilled workers (private sector).	
64	Semi-skilled workers (public sector).	
65	Miners	This category includes foremen and apprentices, but not office workers or workmen who are not specifically miners or quarriers.
66	Sailors and fishermen	Employees only. Includes only persons engaged in specifically sea occupations but not the Navy.
67	Apprentice workers	Category reserved for apprentices actually employed in private industrial concerns, learning a manual occupation (an apprentice salesman, for instance, does not appear in this group, but among salesmen). Nor does it include pupils at technical schools, who do not belong to the active population (like pupils in general).
68	Unskilled workers.	
7–	PERSONAL SERVICE—	
70	Servants (resident)	All domestic servants in private households, e.g. private cook, private chauffeur, etc. . . .
71	Housekeepers (paid, non-resident)	
72	Other personal service	Excluding protective services. Including e.g.: Taxi-driver, waiter, steward, janitor, elevator-operator, etc. . . . barber's assistant, etc. . . .
8–	OTHER CATEGORIES—	
80	Artists, including musicians	This category includes artists in the broad sense of the term (painting, sculpture, music, theatre), as well as certain craftsmen in artistic crafts, such as: art engraving, stained glass windows. It also includes professional sportsmen.
81	Clergy.	
82	Army and police	Members of lower rank of the professional armed forces and the various police services. This category does not include conscripts, who are not included in the active population. Officers and senior members of the police services are classed under No. 35.
9–	NON-ECONOMICALLY ACTIVE POPULATION—	
91	Students and pupils of 14 years and more.	
92	Military recruits.	
93	Former farmers.	
94	Retired from business	Excluding retired farmers.
95	Retired from the public sector.	
96	Former salary earners of the private sector.	
99	Other non-active persons.	

Note about family workers: their socio-professional category is that of the person they are helping. As regards artisans' and tradesmen's wives, in particular when they are helping their husbands they may be declared as unpaid family workers or as proprietors. In either case, the socio-professional category is the same, but their status is different (1 or 2 in the latter case, 3 in the former).

In the tables that go into less detail, the active population is split up into socio-professional "groups", corresponding to the first digit in the code number given in the above table.

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Appendix III

Socio-professional Categories and Status (Contents of the Categories)—(contd.)

Status

Code No.	*Designation*	*Comments*
0	Special status*	Number reserved for the clergy.
1	Own account	Does not include "workers working at home" classed under 6; workers on own account can work with unpaid family workers.
2	Employers	Employers of paid employees (it should be borne in mind that family workers are not paid workers).
3	Family workers	Are defined above in the text.
	Note that in broader re-grouping, unpaid family workers are placed among the employers.	
4	Apprentices	Apprentices in the workshops. This group is analogous to socio-professional category 67 (see above except for the two following points: it includes apprentice-employees but not apprentices in the state service or administration, classed under 7 or 8).
5	Employers of private establishments.	
6	Workers at home.	
7	Employees of public services	By definition includes a certain number of concerns whose employees, while not civil servants, nevertheless have a status that distinguishes them from private enterprise: nationalized railways, nationalized gas and electricity boards, nationalized banks and insurance companies, social security, national coal mines, and some other concerns of the same nature. Does not include Renault nor Air France, nor the nationalized shipping companies.
8	State employees and employees of local communities	All state employees (civil and military) and employees in the Departments, communes and public institutions attached to them (hospitals, etc. . . .).
9	Unemployed	Persons out of work but seeking employment (this group is much larger than that of "unemployed on the dole" who, before they can draw unemployment pay, must answer to certain conditions).

* Number used in special cases, for example for the clergy.

Discussion on M. Brichler's Paper

Mr. B. Benjamin: I should perhaps begin by apologizing to M. Brichler for not being able to address him in French. When the President suggested that I should ask M. Brichler to read a paper to the Society I complied with very great pleasure because M. Brichler is an old colleague of mine on the European Working Group on Population Censuses which has been set up by the Economic Commission for Europe. This is a sort of European workshop in which we hope to gain some knowledge of each other's census practice and to arrive at some degree of comparability in our methods and in our results. In this Working Group M. Brichler has been quite a dominant figure; he has won the respect of all by his clarity of thought and his pursuit of these very difficult problems of classification.

I should begin, I think, by explaining one thing which may not be clear to an English audience. The term "socio-professional" in French has no exact English equivalent because in French the word "professional" means something broader than "occupation" does in English. The nearest equivalent we have, and which we have always used in the European Working Group, is the term "socio-economic group", and when I refer to socio-

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economic groups I am trying to refer to the same thing as M. Brichler's socio-professional groups.

What should be the purpose of socio-economic groupings? The author agrees in his paper that there is no single answer to this question because what may be suitable for estimating political attitudes may not be suitable for measuring differences in expenditure, or patterns of mortality. If there is no single answer what should the population statistician do? He could, of course, adopt a practice of producing a very large multi-dimensional cross-tabulation of the population which would involve all conceivable criteria so that any particular worker could integrate the cells of the tables to arrive at the classification needed for a particular purpose, or he could produce a general purpose classification, not specific to any single purpose but nevertheless useful for most purposes with some tolerable degree of deficiency which is deliberately accepted as expedient. The first course of producing very detailed tabulations is irresponsible and wasteful because these gross tabulations are expensive and in any case they could never be fully comprehensive because there is a limit to the number of criteria that can be encompassed in practice. It is also common experience that all-purpose inquiries, if they do not collapse under their own weight, at best end up by satisfying very few people. We must, therefore, reach a compromise.

M. Brichler indicates, quite rightly, that one should combine a limited number of criteria satisfying the following conditions: first of all, they should represent concepts which are capable of precise definition and are popularly acceptable. For example, "age of termination of full-time education" would be preferable to "level of education attained". "Personal income" is not so satisfactory; it is not so easy to define and it is probably not acceptable in a general population census as distinct from a special household budget inquiry. Secondly, these concepts should achieve the widest possible separation of levels of living environment, and other economic factors (for example, dependency) coming within the concept of a general-purpose classification. Here one might refer to the use of discriminant analysis which has recently been suggested by Kathleen Gales (*Applied Statistics*, 1957, Vol. 6, 123) especially if it is restricted to strictly objective data.

Finally, the criteria should be objective and should not involve subjective notions of social prestige, status or position etc. Thus, although the social economic groups of the British census of 1951 do in fact approximate to the European Working Party scheme, they were constructed from the standpoint of social position or prestige and were not derived automatically by cross-tabulation. It is the automatic character of the new grouping which is important. In this connection it is obvious, of course, that modern electronic machinery would permit simultaneous combination of more than the four axes which have so far been suggested. There is scope for considerable experimentation.

We have now escaped from the assignment of entire units of the occupational classification, such as was adopted in the British concept of social classes, to a classification of individual persons. Not to distinguish between members of the same occupational group on grounds, for example, of employment status (whether working in a supervisory or a subordinate capacity), is clearly indefensible. But this gives rise to a new difficulty. If a multifactorial grouping is to be adopted for the study of social differentials in fertility or mortality, birth and death registrations must be distinguished socio-economically in the same way as the population is classified. This raises the question of whether the public would accept a large battery of questions on registration of births or deaths. In any case, would the informant know all the answers? It would therefore seem that there is a limit to the number of axes which can be combined in the classification.

The author has stressed that he has not linked the classification directly to any particular sociological theory. This is wise because sociological theory must lag behind the provision of facts about social differentials. I would prefer a system which does not prejudge the situation by saying that group n has a *higher* status than group $n + 1$, but simply says that there are clear economic differences between group n and group $n + 1$. This is an important difference between the old social classes and the groups now suggested.

I am particularly grateful for the full account of uses to which the French classification has been put. The examples given are diverse and the tables show interesting gradients.

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Social workers are often shy when it comes to justifying the use of these socio-economic classifications but they must be very sure about their use if they are going to expect any help from statisticians who are subject to severe controls on public expenditure and particularly where public confidence is involved.

I have very much pleasure in welcoming M. Brichler as a colleauge to this meeting and proposing a vote of thanks to him for his paper.

Mr. P. R. Cox: It is a considerable honour to second this motion of thanks to M. Brichler for coming from France and presenting this illuminating paper on a novel subject. I can assure him that interest in the classification of persons by the type of their activity in the economy is at a high level in this country. According to a paper* read to the Society within the last two years, at least five million people are to be found watching their television sets in the evening, and when the programme "What's my Line?" comes on the number is probably higher still. The object of this panel game is to determine the occupation of the challenger by a short series of questions, which may be answered only by "Yes" or "No", and each question relates to some kind of occupational classification; ideally, successive questions should refer to sub-classifications of previous ones. I was listening to this programme last night and was interested to see how experienced operators begin to set about this task. They inquire, for instance: "Do you work indoors or out?", "Do you use your hands or your head?", "Do you make something or render a service?", "Do you work alone or with other people?". These are not the kinds of question that are normally asked in a census, and this serves to underline Mr. Benjamin's remark that the nature of the classification must vary according to the purpose of the inquiry.

M. Brichler recognizes that the type of classification needed to bring out the maximum variety between population groups differs for different inquiries but stresses the advantages of having a constant classification. It is good to see that the approach to the selection of the occupational categories has been an empirical one. Trial and error seem essential in order to find the best possible arrangement, and preconceived ideas can be misleading. It would be interesting to have details of the trial calculations; it would then be possible to know how much was lost in particular inquiries by using a constant classification instead of an adaptable one. Have any particulars been published? The author has mentioned the name of M. Porte in this connection, but the only work attributed to him in the bibliography appears to be on a different subject.

If a constant classification causes loss of information when applied to inquiries in one country, how much more must be lost when we come to international comparisons? A classification that in one country divides the population into groups of roughly equal size may in another give very unequal results. It is useful to have in Appendix II an indication of the relative numbers in the various categories that the French grouping gives, but it would be interesting to know also how the results of applying the proposed international groupings to the French population would compare with those figures. It appears likely that No. 13 on the International List, that is, the category of foremen and skilled and semi-skilled workers, would take up a large part of the occupied population, the remainder of it being spread over sixteen other categories. This is, not, perhaps, the most desirable arrangement possible.

When an international classification is finally agreed upon it will be important to define very carefully the equivalent words in the various languages. We must be sure that we are all talking about the same thing. In this connexion I am rather puzzled by the expression "average personnel".† Teachers and civil servants seem to figure largely in this category, but they might not be very happy to be described in such a manner. The "average" man is no doubt a statistical fiction. Probably something has been lost here in the translation from the French?

I am glad that M. Brichler has stressed the need for statistical comparability between successive censuses. While fresh ideas are being introduced in censal analyses it is desirable that the results of new classifications should be given alongside the comparable results

* "The television audience in the United Kingdom". By B. P. Emmett. *J.R. Statist. Soc.*, **119**, 284.

† "Average personnel "has since been altered in the Paper to read "intermediate personnel".

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based on the concepts formerly in use—just as the Registrar General for England and Wales shows his new socio-economic groups alongside the old social classes. This is particularly important because the technological revolution now in progress is proceeding so rapidly that much can happen in ten years. Electronic computers, automation, atomic energy and nuclear fusion are bound to cause big changes in the occupational classification of the population. Indeed, in Great Britain Class I has already increased greatly in size between 1931 and 1951.

Among the groups in the proposed International List that may give rise to some difficulty in practice are those of employers, workers on own account, and directors; the borderlines between these categories are so ill-defined that it may be advantageous to combine them in order to reduce the possibility of error.

In extending the scope of census inquiries it is probably better to move rather slowly. When there are to be improvements it is a case of which of the desirable developments should come first. M. Brichler has chosen the classification of dependants according to the status of the person supporting them. In Great Britain it is already the practice to group wives according to the occupations of their husbands, and children according to the occupation of their fathers, and to measure mortality differences accordingly; but the extension to other dependants is difficult while the census schedule is in its present form. Radical changes may be necessary in order to carry out what he suggests. In these circumstances, it is worth considering whether we should choose some other improvement in scope rather than introduce special inquiries about dependency. One that appeals to me is the idea of duration in time—how long people have been retired, how long unemployed, how long married, and so on. In this way the age at retirement could, for instance, be deduced.

To conclude, I should like to refer to a point that, strictly, does not arise from the paper but which certainly arises out of it—the question of how to interpret the results of social and occupational classifications. Suppose that social class differences change over a period of time. In what kind of comparison are we interested? For instance, in countries where fertility and mortality have fallen rapidly, the absolute differences between the rates for the social classes have often been greatly reduced but the percentage differences between them have remained much the same. Has there been a decline in the differentials or are they to be regarded as having remained constant? Various interpretations have been put on the figures. If we are to have a development of socio-economic classifications it seems that we should be clear how we are going to interpret the results when we have them.

The vote of thanks was carried unanimously.

Mr. LYDALL: I think this is a most interesting paper but I should like to offer one or two criticisms. Unfortunately the people who take population censuses are not really able at present to give us the data we need for a true socio-economic classification. Since they are limited in the kind of information which they are allowed to collect they are forced to make the best of what they have by using the data on occupation and industry. But we are now reaching the stage where we should try to extend the range of information collected in censuses, especially to the inclusion of data on income.

As many people here may know, the Americans have already collected information about income in their censuses on at least two occasions. If we take the view that "what America can do we can do better", then we should begin to think seriously about including questions on income in the next census, or at least in the census of 1971. If market research agencies continue to develop their work at the rate at which it has been growing in recent years, we should not have much difficulty in getting people to give us their incomes by class intervals.

When one examines the tables presented in the paper it is obvious how much we are handicapped by not being able to break down the population by income as well as by occupation. To a large degree the proposed arrangement by socio-professional groups is an arrangement of the groups in order of their average income; but the dispersion of income within each group is still very large, and it is impossible to tell how far the differences

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in behaviour or attitude between socio-professional groups is a reflection of differences of income or of occupation, or of both.

The work that has been done on this classification is admirable as far as it goes; but until we have income data in addition to occupational data I do not believe we shall have the basis of a satisfactory socio-economic classification of the population.

Mr. McCarthy: I should like to add my tribute to those already paid to M. Brichler for his paper and for his work as Chairman of the Group which, under the auspices of the Conference of European Statisticians, has been considering these problems. I am glad to be able to contribute to the international nature of this gathering. Though what I am going to say is coloured by our thinking at the Irish Central Statistics Office, my remarks here have no official status.

I believe that in a small country the grouping of the population by some sort of rubric such as the socio-economic classification is of particular importance. The British Registrar General can prepare, as he does decennially, a report on the mortality of individual occupations but anything like that is quite impossible in my country since the numbers of people are so small in the various groups. In studying such problems, which involve social and occupational differentials, we must have larger and less homogeneous groups to work on. For this type of inquiry we have used the socio-economic groups and I should like to stress what Mr. Benjamin said regarding the importance of reconciling data from different sources in such work. We have endeavoured to develop infant mortality rates for the different socio-economic groups and have run into some difficulties. We found it necessary to correlate the birth and death registrations to make sure that our data were comparable. In many cases we found rather different information on the birth form from that on the death form. It is therefore particularly necessary when using such socio-economic grades with material from two sources to ensure that the basis of the classification is the same in both cases.

In the Census of Population, 1946, we also used a socio-economic grouping in connection with the analysis of fertility and in the 1951 Census we classified the whole population, and not only the gainfully occupied, by such criteria. We got very distinct differences between the different categories in respect of many items but I do feel that the census analysis itself is only a secondary consideration in the preparation of these groups. Their primary object is to provide at a Census basic bench work data for various inquiries, such as sampling investigations. When we did a household budget inquiry in 1951–52 we related our sample to socio-economic groups, as well as to size of family and region, and used the information we had to correct the differential responses in the different categories. We also discovered in that inquiry that there were substantial differences between different socio-economic categories even at the same income level; the difference between the white collar and the manual worker categories of persons earning the same income was quite marked.

I agree with M. Brichler that the approach to making the classification must be empirical. I look upon the problem as one of dividing the population in strata for the purpose of social inquiries and, ideally, one would want to be assured that the variability within the chosen strata would be much less than the variability between them. At first sight it seems rather improbable that any single grouping of the population would be found which would achieve satisfactory results in the multiplicity of inquiries which might be based on it. Nevertheless I feel that the socio-economic grouping does provide such strata which can be used for most inquiries and that it is bound to be extremely useful.

I particularly welcome the opportunity to attend this meeting and hear views from people in disciplines other than Statistics. One would like to extend the basis of this classification to criteria beyond those of occupation and economic activity which have hitherto been used and in this one would welcome the views of sociologists and others. I am inclined to favour the use of level of education and of income. I am afraid, however, as a statistician, that in my country at any rate, we have little hope of being allowed to

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include a question on income in our Census of Population and, even if we did, of getting correct information from it.

There is one point, which does not arise directly out of the paper, which I mention since it was very much stressed in the recent report of the Conference of European Statisticians and that is the question of classifying the inactive population with the basic object of subdividing people who, though economically inactive, have a personal source of income, from those who are dependent on the activities of other persons. We already subdivide at the Census the inactive population into various categories, but to conform with the objective of the working group one would need to put on the census paper a question asking for information regarding the income of all inactive persons. One can characterize the inactive population, as housewives, children, persons in asylums or other institutions, retired persons and so on, and thus arrive at an approximate answer to such a question in respect of all but about one per cent. of the whole population. Quite obviously one is not going to put a special question on the census paper in order to classify accurately that residual group according to whether or not they are in receipt of a personal income. I therefore believe that we would prefer to continue the present arrangement for arriving at the number of dependants by the traditional process of classification and not to include a special question for this purpose in the Census schedule.

Mrs. L. STEIN: May I add my thanks to M. Brichler for his interesting paper and also add a plea for collecting other information than current occupation in censuses and for using it fully.

M. Brichler says that the French socio-economic classification is not linked to any precise sociological theory; but surely it *is* based on general sociological concepts? He also says that the defects of procedure when members of a household are classified socially and economically according to the current occupation of the head of the household can lead to errors, and lists some of the defects. Obviously in work relating to women or children, in which some of us are engaged, the defects of this kind of classification become extremely marked. In all censuses information is collected about all the members of the household and M. Brichler says it will necessitate a more detailed study of the structure of households in the next census. An analysis of the structure of households should break away from the purely numerical terms which only give us figures of the size of households, regardless of whether they include one or two generations, whether they are old, middle-aged or young, whether with or without young children. I am not here suggesting the collection of new information but the fuller use of information which is already routinely collected in any census. If we wish to arrive at a more meaningful classification of socio-economic groupings, taking into account the structure of families, we could make better use of the information already collected.

M. BRICHLER subsequently replied in writing as follows:

In reply to Mr. Cox, the final form of the French classification, which was—as I have said—the work of M. Porte, was reached by trial and error in several stages. After the establishment of a first draft, the attempt was made to reconstitute the corresponding categories retrospectively, using punched cards from the population census of 1946; this work was attended by many difficulties, because the data given on the punched cards were often insufficient. The classification was then further perfected in various sampling investigations in overcoming the practical difficulties encountered and examining the results obtained. This work of "polishing", which lasted for several years, was not recorded, but if Mr. Cox wishes it I will have enquiries made and send him notes of the three or four drafts tried in turn, before the definitive classification was adopted.

A trial of the draft international classification is to be made in France on a sample drawn from the data of the 1954 census for one particular purpose; we shall have the results towards the end of this year.

In more general terms, it is quite certain that the disadvantage of an international nomenclature is that the necessity of taking into account the different conditions of various

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countries makes it necessary to look for the "common denominator", and this clearly results in a loss of information for each country taken by itself. In point of fact, it is understood that an international nomenclature must be considered as a frame into which each country must introduce subdivisions for its own needs.

Furthermore, experience with international nomenclatures which have been in use for some little time shows that the modifications which are necessary give rise to nomenclatures which are progressively more and more detailed, and the countries concerned slowly adapt themselves to the general standards in use.

Finally, as regards the category "average personnel", which does, it is true, comprise primary-school teachers and a good number of government servants, I think that the difficulty is one of translation. In French, the name given to the category is "cadres moyens", the important word being "cadres", for it implies the idea of command or of research work or organization, and, in general, it presupposes a certain minimum level of education. These "cadres" are divided into two groups: group 30 (cadres supérieurs) and group 40 (cadres moyens).

It is quite true, as Mr. Lydall says, that a knowledge of incomes would be very useful, but we all know the psychological and practical difficulties which this meets with; moreover, to the best of my knowledge those countries which ask for such information receive a good number of "not stated" entries, and also, in all probability, a good proportion of false answers.

Moreover, even if we regularly had information as to the income, this would still not do away with the use of the social-professional groups in dealing with behaviour studies; it is only too plain that if, on the one hand, one social-professional group can cover a wide range of salaries, the converse is also true. In a given relatively narrow range of salary scale, it is possible to find persons of very different social background. For example, in the same range we might well find members of the liberal professions and business men, teachers and craftsmen, clerks and workmen, etc.

In other words, a knowledge of the income might well supplement the social-professional grouping, but this latter would still be quite indispensable.

With regard to Dr. Stein's point, it is true that a more detailed knowledge of the structure of households does not, as a general rule, necessitate extra questions in the census forms; it is, in fact, only a question of extracting the information from the records obtained. Examination of such a kind is being carried out in France on a 1/100 sample of households. The results will probably be published at the end of this year.

As a result of the ballot taken during the meeting the candidates named below were elected Fellows of the Society:

Eric William Beesley
William Eugene Doherty
Josua van Embden
Peter Eugene Francois
Robert Wasson Grant
George Herbert Hudd
Julian Louis Jarvis
John Lewis Jeffreys
Leo Katz.
Dermott Frederic Kelly
Peter Francis Maher
Charles Mair
Paul Henri Mann
Jean-Claude-Gerard Mathieu
John Woodrow Paris
Robert Wilfrid Pender
John Christopher Roots
John Frederick Smithson
Raymond James Tassini

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Research in Statistics

By M. G. Kendall

Research Techniques Unit, London School of Economics and Political Science

[Read before the Royal Statistical Society, Wednesday, February 19th, 1958, the President, Sir Harry Campion, C.B., C.B.E., in the chair]

Introduction

1. I wish to lay before the Society for discussion a number of ideas on the organization and development of research in statistical methodology. Although research in our subject has been proceeding at an increasing pace for seventy years and we have had a special section of the Society to deal with it for over twenty, we have never, I think, had a meeting to consider the broad issues which it raises. Some of those issues are, in fact, of recent emergence and some, perhaps, have not yet arisen into notice. But it is time for us to review the situation, because certain of these issues are important for the future of our whole subject.

2. For the purposes of the argument I require a major premise which should be set out explicitly. I shall assume that research in statistics is a desirable thing to encourage. This is trite enough, especially as a pronouncement to this Society, but it is worth saying; for I find that many people who concede the importance of statistical research will not go on to the obvious conclusion that somebody should finance it. There exists a feeling that research is a thing which people should do with the left hand, a kind of by-product of more socially useful activities such as teaching or manufacturing. Such points I develop later. The matter is mentioned here by way of introduction to make my standpoint plain.

3. Miss Kathleen Gales has recently conducted an inquiry among the Fellows of the Society concerning the utilization of statistical methods. Her results will be published in separate reports, but I have been able to draw on some of them to provide a background against which we can present our research problems. With her permission I quote the following material.

Part 2 of Miss Gales's questionnaire, which referred among other things to research, was answered by 910 Fellows of the Society. This is not a complete set of all statisticians in the U.K. who are doing, or are interested in, research, but it doubtless includes most of them, and for my purposes the analysis of those 910 replies gives a sufficiently representative picture of the situation.

4. By employment, the distribution of responses is given in Table 1.

The first two questions correspond roughly to the two items on which the Research Section is based, new techniques and the application of existing techniques in new fields. Neither gives us an idea of how many people are engaged in these activities at any one epoch, or how much of their time is spent on them. The third question throws some light on the point. About two-thirds of the 134 University workers (say 88) were engaged on a research project when the inquiry was conducted; about 40 per cent. of those in medical and scientific research (say 44) were so engaged; and elsewhere about a quarter (say 170) were so engaged.

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TABLE 1

Q_1 = "Do your official duties involve the development of new techniques?"
Q_2 = "Do your official duties involve the application of known techniques in completely new fields?"
Q_3 = "Are you at present engaged in any specific research projects in statistics?"

(1) *Employment*	(2) *Numbers*	(3) *Affirmative Q_1 as % of (2)*	(4) *Affirmative Q_2 as % of (2)*	(5) *Affirmative Q_3 as % of (2)*
1. Civil Service	158	22·8	60·1	24·1
2. Public Corporations, etc.	113	24·8	52·2	27·4
3. Medical, scientific, administration and research	110	35·5	54·5	40·0
4. Universities	134	51·5	54·5	63·7
5. Commerce and finance	111	30·6	39·6	23·4
6. Manufacturing industries	242	35·1	56·2	26·4
7. Miscellaneous or none	42	14·3	26·2	35·7
8. All employments	910	32·6	52·5	33·6

5. The fields of interest of the 910 Fellows are indicated in Table 2, which shows the answers to the question: "In which fields of applied statistics or statistical theory are you most interested in doing research at the moment?"

TABLE 2

	Numbers	*Percentage*
Stochastic processes, Monte Carlo methods	56	6·2
Time-series analysis	20	2·2
Prediction, estimation, tests of significance	18	2·0
Other fields of mathematical statistics*	94	10·3
Survey techniques	30	3·3
Economic statistics	87	9·6
Industrial techniques	46	5·1
Operational research	46	5·1
Actuarial techniques and social statistics	107	11·8
Psychological testing	8	·8
Miscellaneous others	40	4·4
None or no reply	358	39·3
	910	100·1

* No one mentioned more than four times.

The high proportion giving prime place to actuarial techniques and social statistics may come as a surprise. It is clear that Fellows' interests are very widespread, sometimes in areas which one would not expect from a glance through the major research journals.

6. I shall not lean very heavily on these figures, or anticipate what Miss Gales may have to say about them. They are sufficient to illustrate the very varied subjects covered by statistical research and the very varied types of employment of the individuals who are doing it. In fact, it becomes apparent that before going further we should discuss what we mean by research in statistics.

7. In an arts subject research can mean very little more than postgraduate reading and essay writing, the kind of work which gave rise to the well-known observation that copying from one book is plagiarism whereas copying from two books is research. In the sciences the situation is different. A student can still do research without originality merely by working out an idea presented to him by his supervisor, but at least he has to work it out

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and produce some new facts. In statistical work (to use the expression in its most general sense) one finds both types, but comparatively few of the first. It is possible to produce research of a useful but pedestrian kind; for example, by constructing a White Paper on National Income for Ruritania on the model already laid down for the United Kingdom. In my experience it is relatively rare even for studies of this kind not to run up against some novel theoretical problem requiring an original attack. And when we come to research in statistical methodology, of course, novelty is the essence of the work. It is what I may be allowed to call genuine research.

8. In passing we may notice an important practical problem created by these different grades of work. A "research assistant" may be little more than an amanuensis digging into records for his Professor; he may be doing genuine research by working out an idea given to him; or he may be doing genuine research by working out his own ideas. To put all these people on the same salary scale seems a gross injustice; to discriminate can involve some embarrassing critical judgments of the relative importance of work done under different supervisors. The best solution we have been able to find at the London School of Economics is to create two grades of research assistant, junior and senior.*

9. During the past twenty years there has been a revolution in the nature of the milieu in which statistical research can take place. The days of the solo worker are gone. At one time it was possible, perhaps, for an individual to shut himself up in his study and excogitate fundamental ideas, even working them out to the point of numerical application with the aid of a Brunsviga. This is no longer so. The research worker nowadays needs access to an enormous volume of literature and, very often, to computing aids which are quite beyond his private means. He has to spend more time earning his living than some of his forbears. He also needs quick communication with men engaged in work similar to his own and with the fields in which his work can be applied. What used to be done by correspondence and occasional visit now has to be done by personal discussion and frequent conference.

10. Even before the war the mention of statistical research called to mind the two centres where workers were forming into groups, University College and Rothamsted. Nowadays it seems to be essential to think of research teams, not of individuals. The team is the natural unit. I do not mean, of course, that individual work is to disappear, or that every research worker must hunt with the pack. In a small country like ours most people who feel a sense of isolation can establish some kind of liaison with a research unit. But in general the isolate works under great handicaps and his mere existence reinforces the necessity for research centres to which he can, however loosely, attach himself. My conclusion is that we must foster the development of research units.

11. A natural consequence of the formation of such units is the appearance of the professional research worker. In universities this type of individual is something of a novelty. The tradition is for research to be done by teachers; and although the relative amounts of a man's time which he gives to one or the other have always varied a great deal according to University, department and individual, it remains true that most university salaries are paid for teaching. Research is something a man is expected to do when his other duties are discharged. (The existence of a few research chairs does not seriously affect this statement.)

* The junior grade has salary £450 × £25—£525 and the senior grade £700 × £25 × £675. Above the grade of research assistant we also have Junior Research Officer (£650—£900 + £60 L.A.), Senior Research Officer (£950—£1500 + £60 L.A.), and certain special posts of Research Associate or Research Fellow.

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That there should be, *at all levels* in a University, staff who are paid to do research, and may do a little teaching in their spare time, is a concept whose full impact on the University has not yet been felt. In fact, I dare say that some people would argue that it is inconsistent with the true nature of a University.

12. This brings us to the question of the relationship between teaching and research in the modern University. (The problem does not arise in business or administration, except perhaps in a few areas like London, where research workers sometimes teach during the evenings in technical institutes.) It may be argued with great cogency that the two should not be separated. The common opinion, which I share, is that a research worker ought to keep closely in touch with students, both for his good and for theirs. The hiving off of separate institutes, whatever its virtues in other subjects, has not been much attempted in statistics, and when attempted, has not produced much methodological research. Teaching and research should, one feels, go hand in hand to their mutual benefit.

13. Unfortunately this principle has been widely misapplied. It has meant that nearly all academic appointments are to teaching posts. It is true that university teachers need not know anything about teaching and that many of them do not; but they have to try. It surely must be plain that ability to teach is not necessarily correlated with ability to do research; and an inclination to do one may very possibly be negatively correlated with an inclination to do the other. In my opinion this ought to imply that all the appointments in a University ought not to be to teaching posts with the expectation that the incumbent will do some research. Some of them should be to research posts with the expectation that the holder will do some teaching.

14. Indeed, the point may be pressed home. It is widely believed (and is also, I think, true) that an advanced teaching institution where no research is carried on develops an inelasticity leading ultimately to sterility. It seems to follow that definite provision should be made for research and definite appointments recognized as ranking equally with teaching in claims on the institution's financial resources. This, with some exceptions, is precisely what does not happen at the present time. When a University is faced with financial pressure (and in future it always will be) the teaching posts are filled first. Research has to take what is left over. If there is nothing left over, so much the worse for research.

15. In consequence, a good deal of research in a University at the present time is financed either from a few scraps which are left over when teaching and administrative commitments have been met, or from charity. A director of research is in some danger of becoming a professional mendicant, appealing to foundations or to industry for money to support his work. My own research unit was founded entirely on money from the Nuffield Foundation, and in the present year about 80 per cent. of my budget comes from non-university sources. My benefactors will not think me ungrateful if I say that such a situation has serious defects. Grants from outside sources are necessarily limited in time; a foundation may be very willing to launch a project and support it through its childhood, but cannot allow it to become a permanent pensioner. This seriously affects the continuity of a research unit. Nowadays a competent man in statistics can earn much more in industry or commerce than in research in a university; he may be so dedicated to his subject that he is willing to accept a lower academic salary in return for greater freedom and time to work out his own ideas; but the least he can expect is some guarantee of continuous employment and some prospects of promotion. This cannot be provided. If his institution has no earmarked provision for research he may never know for more than a year ahead whether his salary

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is assured; and if he is being carried on a foundation grant his appointment is rarely for more than five years.

16. The solution to this difficulty seems to me to involve an element of compromise. I conceive of the research unit as an atomic sort of institution: it should comprise a stable nucleus of full-time research workers borne on the institute's vote with full academic rights and, if necessary, with an earmarked appropriation. Circulating round this nucleus there may be temporary staff, attached by bonds of varying strengths, appointed for *ad hoc* projects and financed, perhaps, from outside sources. The key to the whole is the stability of the nucleus. Without such a thing research is bound to be unsatisfactory. I speak with less assurance of the similar problem in industry, but I suggest that the nuclear principle still applies. There should be a permanent, though perhaps small, staff on the establishment; particular projects may be taken on but will, of course, as a general rule, be financed by subvention from within the firm and not from outside.

17. It is of some relevance to consider how large a statistical research unit should be. No single figure can be put forward to answer such a question. There are, nevertheless, some numerical factors in the situation. For example, there seem to me to be clear indications as to the size of the team working (in a directorial capacity) on any project. Four I regard as the upper limit. Three is better and two, or even one, quite permissible. I regard the research project as analogous to the old merchant venture. One needs a good crew and some reliable senior officers, but only one captain to each ship.

18. The unit from which the teams are chosen can, of course, be much larger. In research, however, as in factory management, it is important that no organization should become so large that the director is out of touch with any member of it. In particular, the director should be able to discuss any project with any member of his staff. This elementary requirement imposes very substantial limitations on the size of the whole unit. At a guess I would say that the limit is in the neighbourhood of twenty (I am speaking, be it understood, of the expert personnel, not of the subordinate services such as typing and computing.) Beyond this the unit is in danger of losing its individuality. It is rare for units of such size to exist in universities, at least in statistics, and the problem of directing and controlling them has not yet become of any great importance. But it may well do so as time goes on. One direct consequence of this is that no one unit can hope to cover all the fields of interest referred to above in section 5, and we should try to concentrate on a few fields within any one unit. Another consequence, as I see it, is that we need more units.

19. Up to this point I have said nothing of the research *student*. In some subjects the student makes quite a big contribution to the volume of research in progress. In the past, and to a smaller extent at the present, this is true of statistics. But recently the supply of research students has been declining. Most of mine come from overseas. Our own graduates are still mostly taken from us by military service (surely one of the greatest stupidities of the time) or are attracted into employment at rates two or three times what they would have to live on for three years to obtain a Ph.D. This is unsatisfactory, for the supply of research workers in training is correspondingly declining. I have not yet found a good solution to this problem.

20. It might also be remarked that we suffer in academic life from three types of student who assume the appearance of research workers as a kind of protective mimicry. There is first of all the man who, on graduation, cannot face the cold world outside and would like to stay encloistered in a community which will not judge his efforts, or lack of efforts,

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with a utilitarian eye. Secondly, there is the student who would like to do research but has no idea on what topic; this individual invariably thinks that perhaps his supervisor can suggest something. Thirdly, there is the careerist who wants a postgraduate degree because it will get him a better job.

21. I have little sympathy with the first type. Some of my colleagues have none for the other two either, but I think they deserve some consideration. So far as concerns the second type, we must remember that in statistical methodology it is exceedingly difficult to find a suitable subject for a research thesis. Nearly all the easy problems have been solved. On the difficult ones a student may easily spend two frustrating years without making any headway. In any case, he can hardly be expected to propound his own problems immediately on graduation, because all good research problems arise from practical experience. He can, nevertheless, be rather a trial. Many a supervisor will furnish a problem and have it returned to him unsolved after the lapse of a year or two; and this involves locking up a considerable amount of intellectual capital.

The student who regards his Ph.D. period as one of postgraduate training is also entitled to consideration even if he wants his degree for commercial purposes; especially if he is contemplating working abroad. In this country a research degree (in statistics) does not carry much weight whereas in the U.S.A. the possession of such a qualification may make a difference of $2,000 in annual salary. An honour has profit in other countries if not in our own.

22. Perhaps matters will improve in time, but for the present I have ceased to regard the doctoral degree as making any substantial contribution to actual research or, what is more important, to the training of research workers. It seems to me that we shall develop towards a four-year period of primary training (either four years for a degree or three years plus a Diploma year), at the end of which those persons who wish to do research will obtain jobs as research workers. The importance of the research unit as a training ground is thereby much enhanced. It takes over part of the postgraduate work of the University.

23. To sum up, then; the statistical research unit should be an autonomous unit and should consist of a stable nucleus with terms and conditions of employment equivalent to those of other members of the organization within which it functions. The financing of this nucleus should be carried out by the organization; and this should include the services which the unit requires by way of typing, computing, library facilities and so forth. Associated with this nucleus, and perhaps much larger in size of staff or financial resources, should be particular projects, support for which may come from Foundations, Trusts, Trade Organizations, the D.S.I.R. and individual firms or persons. The nuclear staff and probably most of the more peripheral staff should be full-time research workers engaged and paid as such. The unit may, in many cases, have to train its own research workers: it cannot expect to find a sufficient supply emerging from the University on graduation or post-graduation. Teachers should be encouraged to do research under the aegis of the unit; and members of the unit should be encouraged to teach.

24. From these generalities I proceed to the particular on the principle that every statistical paper should contain a practical example. Unfortunately, the only example I can discuss is that of my own Research Techniques Unit, which in some respects is atypical. However, our experience illustrates a good many of the points and may be useful to others, it not as a guide, at least as a warning.

25. The success of any research team depends on the men who compose it. (Here and

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throughout, as the statistician says, "man" embraces "woman".) One sees a lot of potential research impeded or spoilt by the preparation of a good project, the provision of the necessary funds, and then the discovery that suitable men are not available. It is good practice never to put up a research project without being able to name the individuals who are going to run it. In my own opinion I have been very fortunate in the personnel recruited to my own unit, and the whole of our development has been built round their capabilities and interests.

26. One cannot create research workers. The essential capacity is born, not made. But one can create an environment in which they thrive. For example, we have found it necessary to set up our own specialist library, which we were able to do through the generosity of the Leverhulme Foundation. We have had to acquire a certain number of machines. We have had to obtain access to computing equipment of electronic types, and this in turn has led us to begin to train our own programmers. We have systematized the issue of our own publications, of which there have been over seventy in the last eight years, apart from books. In some cases we have had to prepare our own bibliographies. All this may sound rather routine and pedestrian, but the point is that it has all had to be done deliberately and carefully with very limited resources, and not just allowed to happen anyhow. It has only been possible because funds were available from outside.

27. The question of bibliography is an interesting case in point. Articles of relevance to statistical methodology are now appearing, I suppose, at the rate of about a thousand a year in over a hundred journals. No individual can cope with a flood like this. Consequently a bibliography, once a convenience, is rapidly becoming a necessity. I have therefore set in train the preparation of a bibliography of methodological statistics from the sixteenth century onwards in all Western languages. We already have on cards about 15,000 titles, and before we are finished there will probably be as many again. In a way it is a most ambitious project; but it is only an ancillary to our main work.*

28. Again, in our research on dynamic economic systems we found that there was a remarkable lack of practical time-series on which our methods could be tested. This will hardly be believed, but it is quite true. Three people (and I hope it will soon be six) are engaged simply on the task of preparing a library of reliable statistico-economic information. Once this is done it will be an invaluable adjunct to all future research in this field. The point is that it is having to be done (on money, this time, from the Ford Foundation) merely as an ancillary service, merely to create the environmental conditions necessary for our research.

29. In connection with this matter of environment I should like to mention one other important background factor contributing to our progress. I have had no opposition to overcome from colleagues inside my own School. On the contrary, they have supported me even when they pretended not to understand what we were doing. When I contrast this situation with some I could mention in more than one other establishment I feel persuaded that our success in founding a research unit is due, as much as anything, to the fact that we are not a part of some other faculty such as mathematics or economics. I dwell on this point to register the opinion, which has become stronger with the years, that statistics will never come into its own in any university until it is recognized as a subject

* Another urgent need is for a comprehensive abstracting service. The International Statistical Institute at Stockholm in September 1957 asked Miss Gertrude Cox and myself to try to start one. Again it is significant that the work was entrusted to sizeable research units and that launching funds were to be sought from private sources.

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of full standing by itself. If we want more research units they must not be hampered by misunderstanding (to put it at its best) or strangled by jealousies and competitive claims from other disciplines (to put it at its worst).

30. Nearly all the research staff of my unit do some teaching, and in fact the teaching and research staff are so closely integrated that we are rarely conscious which is which. One effect of this has been some interesting developments in our teaching methods and postgraduate courses. For teaching we are finding it necessary to write some of our text-books and monographs; for example, on intermediate statistical methodology, on mathematics for statisticians, on sampling method, on time-series and on the analysis of economic systems. A good half-share in this work is being borne by the research staff.

31. Such books are necessary not only for ordinary teaching but as propaedeutic to the training of research workers. Of some practical importance at the other end of the research process is the interpretation of results. Statisticians have often suffered a good deal from an inability to explain to other research workers the general import of their discoveries. The fault was not always theirs but lay at the receiving end. Only recently, for example, has it dawned on the medical profession what people like Greenwood and Bradford Hill were talking about. Certainly in economics and commerce we have this problem. Even to-day I meet economists whose admitted allergy to statistics is based almost entirely on a natural but erroneous belief that statistics is an elaboration of what they had to study under that title as part of their primary training in economics.

32. In our work on the dynamics of economic systems this exegetic problem is very serious indeed. Dr. Phillips and his colleagues, moving freely and familiarly in a world of electric networks, transfer functions, multivariate autocorrelation and servo-mechanisms, are creating and speaking a language which is Minoan Greek to the ordinary economist; and it is difficult to convince the latter that they are doing much the same as he but with a shift of emphasis and with the aid of much more precise tools. Special efforts have to be made to bring the results down to earth for general consumption. (We resist that kind of scientific snobbery which gains prestige by incomprehensibility.) One does not need a public relations officer in research; but a hermeneutic agent of some kind seems essential.

33. Connected with this aspect is the relationship between a research unit and the outside world. I have claimed above that every good research idea originates in an attempt to solve a practical problem. It follows that a research unit must be prepared, if necessary, to pursue its work in practical spheres. Especially is this so of social survey and operational research. Feeling this to be so, I have always tried to establish links between the university and the consumer of statistical method. For example, our work on survey methodology has been supervised by an informal body of members drawn from market research organizations. This arrangement has worked fairly well but it is compelled to stop short at one point which, I feel, is important to the relationship between Universities, the Government Service and industry. There ought, one feels, to be some system under which workers in one branch can transfer for a time to another. There must be many research workers who would like to go out and work, say, in industry for a time to recharge their intellectual batteries and collect some new ideas. Likewise, it seems probable that there are men in the Government or industry who would like to take a couple of years away from routine to think about what they are doing and carry out some research on ideas thrown up by their ordinary work.

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34. Theoretically such a thing is possible. But in practice it is so difficult to achieve as to happen very rarely. And as time goes on it seems likely to become rarer. The Civil Service recognizes secondment and approved employment in principle, but scarcely ever makes use of the possibility in practice. And no commercial firm does what Guinness did fifty years ago in sending Gossett to University College. One cannot blame them. Nor can one blame an individual who, however much he would like to go, feels that he cannot leave his job vacant without serious risk to his career.

I doubt whether there is any satisfactory solution to the problem. It is, perhaps, a defect of the type of society in which we live that a man cannot, scientifically speaking, don the yellow robe without also taking a vow of poverty which his family commitments must deny to him. As circumstances now stand, a man who leaves research usually must expect to leave it for ever; and if he acquires a taste for it in late career he must usually put his aspirations among the collection of unrealized dreams which we all carry with us to retirement and thence to the grave.

35. My main purpose in presenting this paper to the Society is to raise the subject for discussion. I do not suggest that there is anything that the Society collectively can do or should do about solving some of the problems which arise. It would, nevertheless, be useful if we could agree among ourselves certain principles of procedure; or, in default of agreement, if we could bring to light our differences of opinion. By way of summary, then, and with an eye to future developments, I will set down some of the determinants as I see them:

(1) To cover all the research which is required in the domains mentioned in Table 2 it is necessary to have several research units in this country.

(2) There being natural limitations on the size of a unit, it would be better if each research unit developed some specialism in particular branches. Everyone should be free to tackle any problem he liked; but work would be more effective if particular units concentrated on particular fields. Such a development would probably take place naturally.

(3) Business and industry will probably develop their own units. Research in the Civil Service and Local Government is, I think, almost impossible under the pressure of day-to-day interruption, except for specialized agencies such as the Road Research Laboratory. In Universities the formation of units will probably have to be specially undertaken and fostered. One does not see them growing naturally to the right size unless some dedicated individual is prepared to spend a lot of time making himself a nuisance.

(4) The nuclear part of the unit should consist of full-time research workers with terms and conditions, as regards salary, continuity and prospects, not less favourable than other persons in the organization of which they form part. As regards Universities, there should be a specific appropriation for such a unit, put to the University Grants Committee as such.

(5) If there is any re-introduction of compulsory military service, statisticians, in common with other scientists, should be exempt on the grounds that the last thing anyone will want a statistician to do in a war is to take part in combat.

(6) The functions of the unit must include the training of research workers.

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Discussion on Professor Kendall's Paper

Professor Bartlett: This Society has in recent years discussed more than once the thorny problem of how statistics should be taught; the problem of how to conduct statistical research is just as important and is somewhat complementary to the first problem. With both, one of the first things to try to decide, and probably one of the last that will ever be agreed on, is what statistics, as a subject, is. I am not sure that Professor Kendall entirely faces this issue in his paper, or at any rate gives it sufficient discussion. A lot of what he says is pertinent, indeed very pertinent, to the organization of research in general. More attention has been focussd on this particular problem lately, and in Manchester we had only the other month an illuminating talk on "Research as a Business" by the Director of the Shirley Cotton Research Institute. In universities the logical anomaly, at which Kendall hints, of university teaching appointments being filled by candidates more renowned for their research than their teaching is common to all departments. It is not so illogical as it first appears, but the difficulties which it raises for Heads of Departments are generally familiar; there is no doubt that a more explicit recognition of the place of both research and teaching in a university department greatly eases the organization of both.

What I was thinking of in referring to the nature of our subject is its rather amorphous character, due to its wide applicability. I have no doubt in my own mind what statistics, as a subject, is, but I define it in terms of what Professor Kendall has himself called "the statistical approach" and its subject matter is consequently limitless. Looked at in this way, it touches philosophy, mathematics, physics, biology, medicine, economics, sociology, industry, engineering and commerce; and the well-known organizational question arises in statistics research as well as in teaching : should the departments more explicitly concerned with such subjects have their own statisticians, or should there be central statistical departments or laboratories? Of course, such an amorphous subject as "applied mathematics" has an analogous problem; but expediency and tradition usually dictate what kind of applied mathematics is taught in any particular applied mathematics department, and I suppose statistics departments are subject to rather similar tacit restrictions.

Nevertheless, there is perhaps an especial difficulty for statistical post-graduate students and staff, even if we confine our attention for definiteness to departments associated with my own subject of mathematical statistics and its applications. The importance of the applications is well recognized and so is the importance of keeping theoretical research co-ordinated with applications. But when the applications are so diverse how does the potential statistician begin to study them? I think this problem is particularly acute in Departments of Mathematical Statistics or the equivalent, and even in this country where our tradition for a healthy compromise between pure and applied research is so strong, I believe we mathematical statisticians are partly living on the capital of the experience of those of us who have worked away from the universities, at least for a time, for example in agriculture or in industry or in research organizations, or, particularly during the last war, in government service. I hope young mathematical statisticians who wish to gain practical experience will not be discouraged in any way from moving fairly freely in and out of universities, as they have done in the past; I hope also that at least a large proportion of them, even within the universities, will also interest themselves to some extent in fields of research in which their various techniques can be usefully applied. I do not wish to deride techniques as such, but I am sure Professor Kendall will agree with me when I say that they are of no value unless someone uses them. The increasing specialization within our subject makes it more relevant to utter such a platitude now than twenty or thirty years ago.

Finally, with regard to the actual organization of research in statistics, Professor Kendall has made out a convincing case for the research group or unit. I imagine, however, that there is plenty of scope for variability; in particular, a research team usually works on a project already decided on, and it is still essential to ensure that a sufficient flow of new

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projects and original ideas is emanating from somewhere. One-man teams can still play a vital role here.

I have great pleasure in proposing this vote of thanks to Professor Kendall for his paper.

Mr. FRANK BURCHARDT (seconding the vote of thanks): I am happy to associate myself with this vote of thanks for Professor Kendall's stimulating paper. Basically it is a reasoned and powerful plea for the setting up of more statistical research units, and we can probably all agree that this country is under-endowed with research institutions in many fields and not least in statistics. Tables 1 and 2 give some indication of the high proportion of statisticians interested in developing new techniques, and of the wide diversity of fields of application. But this is only a static picture, and I regret that the paper did not tell us more about the expansion of research in statistics and the growth of research units since before the war. It would not surprise me if the number of statistical research units in the universities alone had increased greatly in this country. Growth rates are a tricky concept, and I for one would readily admit that the absolute level is still too low. Some new units have grown up in response to the needs of particular subjects, and the number of statisticians attached to different branches of study has probably increased very considerably.

Professor Kendall would prefer to have a number of research units unattached to any particular faculty—such as mathematics, economics or biochemistry. There is room for such development provided a close relationship is maintained between the research unit and what he calls the outside world. But this should certainly not preclude the possibility of research units which are more closely connected with the work of particular departments. Some specialization between statistical research units would certainly seem to be highly desirable.

Like Professor Bartlett, I doubt whether the days of the solo worker are over. Like many mathematicians, some statisticians are lone wolves and they should find a place on the teaching as well as the research side just because they do not fit into either. Nor would I agree that every good research idea originates in an attempt to solve a practical problem. There will always be some statistical research which has no obvious immediate application to practical problems, and perhaps our American friends are specializing a little bit too much on the development of statistical theory without much relation to its practical application. This may be partly accounted for by the close relationship frequently maintained in the States between the mathematical and statistical branches of study, and there may be some need to redress this imbalance.

There is only one hint in Professor Kendall's paper on the supply of statisticians. They are in very short supply now, and this deficiency goes back to the school curriculum. Too few pupils are taking mathematics to satisfy the science and arts faculties, including economics, and with the deliberate expansion of the science side envisaged over the next ten years we may well find that we are running short of statisticians of all grades. Unless we have a fairly elastic supply it will be very difficult indeed to fill posts in research units with adequately trained workers.

I am coming now to another basic question raised in the paper, namely, the relation between teaching and research. This is, of course, a problem of reputable age, and has been the subject of controversy in the nineteenth as well as the twentieth century. By and large, the view that teaching is the main function of the universities has held the field in this country and it is assumed that research will come as a by-product of teaching. Much impressive research has indeed been done by people appointed to teaching posts, but I fully agree with the author of the paper that room must be created for research posts in which teaching is permissible but not obligatory, and that the rate of pay of research workers should be equal to that of comparable grades on the lecturer or reader scale. However, that does not solve the whole problem because teaching has a much higher status in the universities in this country, and it is my experience that many good research workers will try to get a teaching post not only because they get better security but also

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greater leisure. The long tradition that teaching posts carry a higher prestige and better conditions than research posts can only be changed gradually.

I am in full agreement with the statements about the size and functions of research units. Good research workers will hardly be attracted by salary alone, but also by the scope and nature of the research projects. It is probably right to choose projects which require working in small teams, and resources that are beyond those of the individual teacher.

Few people will quarrel with Professor Kendall's statement about the financial structure which a unit should have. It is essential that the basic overheads and the salaries of the "nuclear" staff should be provided by the university, while outside funds should be attracted for specialized research projects which may also employ workers taken on *ad hoc*. As to the optimum size of such a unit, much will depend on the nature of the work carried out and the scope and variety of projects undertaken at the same time. For university purposes I would agree that the optimum size is one in which the leader of the unit can be at least in touch with all the researchers about their individual projects: 15 to 20 is probably the right figure; anything beyond that will require some form of delegation and some machinery for maintaining contact among the various research workers.

One final remark. Professor Kendall's unit is providing common services to other research units and to individuals by embarking on a very ambitious bibliography of methodological statistics in Western Europe and also, in conjunction with the International Statistical Institute, on a comprehensive abstracting service of periodicals. In addition, work is in progress on a library for statistical/economic information. These are unique services rendered by the leading unit to all other research units, and to statisticians in general. We and future statisticians are greatly indebted to Professor Kendall's initiative, foresight, and, last not least, financial wizardry.

The vote of thanks was put to the meeting and carried unanimously.

Mr. E. C. FIELLER: There are a great many points in Professor Kendall's paper which I should like to take up, including some I should like to refute.

I read with surprise in paras. 13 and 14 of the paper that University teachers are appointed merely to teach. With still greater surprise I found only last week a University Reader who assured me that his official duties were restricted to lecturing. The general and, I think, the official view is that the Universities are in fact places where fundamental research should be done. I am certain that any University teacher who disagrees, like the Reader I have just mentioned, has either forgotten or has never studied the terms of his appointment.

Doubtless many University teachers feel, as do most of us, that they have not sufficient time for all the research in which they would like to engage. I suspect that for a man at a University this is partly because his lecturing has to be done to a time-table, whereas his research does not, and partly because statistics is still a young subject and somebody regularly lecturing in it is more inclined to spend time completely re-casting his lectures than is somebody lecturing in geometry or hydrodynamics.

University statistics departments are or should be subject to an additional load which other departments do not bear. They should be called upon for advice in many other subjects. Consulting work of this kind I regard as research in applied statistics. It can fertilize the statistician's mind, but it can also absorb a great deal of his energy. In some Universities it is recognized that this advisory commitment entitles the statistical group to some special consideration in the competition for staff. Professor Kendall's claim that the duty to pursue fundamental research also entitles the group to a bonus distribution seems to me to be more doubtful because that is a claim which could be advanced on behalf of any other branch of science.

I share Professor Kendall's regret that there is not more migration between the Universities and the world outside them. In past days the possibility of such migration was theoretically a basic feature of the Scientific Civil Service, but it has rarely come about. This is one of the things which the United States seems to manage better than this country.

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Here the Medical Research Council and the Agricultural Research Council seem in particular cases to have found the best type of solution in placing advisory statistical units, formed from their own bodies, in University Departments, as at the London School of Hygiene and Tropical Medicine, and at Mareschal College, Aberdeen.

Professor NEYMAN (amplified in writing): I should like to congratulate Professor Kendall on his splendid achievement in creating a new centre of modern statistical research and instruction. The post-war activities of the whole group of statisticians at the London School of Economics are most impressive.

The problems that Professor Kendall has brought to our attention are very complex and I believe he is right in insisting that no unique single solution can be found and that there is room for varying points of view. There is likely to be very general agreement on Professor Kendall's premise that statistical research is a desirable thing to encourage. The reason is, of course, that all modern science is becoming increasingly indeterministic at an increasingly rapid rate and thus the services of a statistician are more and more frequently required. The questions on which there is likely to be a difference of opinion are how best to organize statistical research and what is the best form of support. Professor Kendall has made an eloquent plea in favour of a statistical research unit composed of some twenty people and attached to a school of higher learning. While agreeing with Professor Kendall on a number of points, including the usefulness of the research unit as he described it, I should like to propose a few minor amendments and to advance a few suggestions on points not directly mentioned.

(*a*) First, there is the question of subdividing the members of the unit into two categories, teachers and full-time research workers. It seems to me that Professor Kendall is not sufficiently appreciative of his own present unit. According to what he says in para. 30, currently there is no such division of his staff and I think that precisely this is the determining factor of their success. I am convinced that a separation of research and teaching activities would be a mistake. Whatever the official contracts may say, research is a natural prerequisite to the successful career of a university teacher. And, vice versa, a full-time research worker without access to teaching in one form or another is very likely either to desert the institution or to degenerate into a performer of routine activities. Should it be true that, currently, the teaching duties of Professor Kendall's personnel are so extensive as to leave little time for research, my own preference would be to increase the number of teachers who would share in research and in teaching, rather than to employ extra people for research purposes only.

(*b*) I do not agree with Professor Kendall that the days of the solo worker are gone. The best example is, probably, David Kendall who, right next door to us, in Magdalen College, Oxford, does excellent research in statistics without being attached to (or shall I say, encumbered by) any extensive research unit. A glance at a few issues of statistical journals will suffice to indicate the influence on other statisticians exercised by David Kendall.

(*c*) My third point is that, probably, it would be a mistake to concentrate public support on the organization of research units to the exclusion of other forms. I would advocate another guiding principle which seems to have a greater generality of application. This principle is to support talent and to adjust the form of the support to the particular needs of the individual concerned. If this principle is adopted, there will be a substantial diversification of the forms of support.

During my present stay in England I have had the good fortune to have a glimpse at several centres of statistical research not mentioned by Professor Kendall and these provide illustrations of what I have in mind.

I was much impressed with what I saw in Manchester. Here Professor Bartlett has assembled very competent and active personnel enjoying internal co-operation. The papers by Moyal are obviously influenced by Bartlett and vice versa. Also there are contacts with representatives of many other disciplines, and the activities of the group are broad: from biology to corpuscular physics, with many sections of statistical theory in

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between. Here, then, we have an effective statistical research unit of the same type as the one at University College, London. Although it is still a little early to judge, something similar is likely to develop in Birmingham as a result of the activities of Professor Daniels. The best forms of support in these cases may well be the same, but, perhaps, slightly different from the form best suited for the unit of Professor Kendall.

In Oxford the situation is very different. Here, of course, there is an excellent centre of statistical research and instruction with several free minds uninhibited by inbred influences of eminent teachers. But should one visualize Oxford as a seat of one single extensive statistical research unit? I am inclined to doubt this and expect that, for example, Bailey, Champernowne, Hammersley and David Kendall will be more effective in their work if left mutually independent and free to pursue their own lines of interest. This should not prevent them from having a joint seminar whenever the occasion arises. Naturally, they all need support, but the forms of support are likely to be different.

(*d*) My fourth point is connected with Professor Kendall's expectation that "business and industry will probably develop their own statistical research units". Professor Kendall will be glad to learn that his expectations are already realized and that some industrial organizations already have substantial statistical research units. It was my privilege to visit one of them, at the Boulton Paul Aircraft Ltd. at Wolverhampton, and I am full of surprise and admiration. Here is what I found. Some time ago, the director of the company, Mr. J. D. North, realized that some of the company's problems required statistical research. Presumably, he looked around for competent statisticians and, finding none, decided to create them. He employed two mature men, pure mathematicians who apparently had never had any contacts with statistics to speak of, but were talented. With this prerequisite and under pressure of their company's problems, they learned statistics and, since about 1952, have published a substantial number of papers in this country and abroad, all dealing with the most modern branches of our science: probability limit theorems, stochastic processes, theory of information, etc.

(*e*) This brings me to my fifth point, the details of the organization of a statistical research and instruction unit in a university. When a new unit of this kind is planned or when the leaders of the existing units have an opportunity to re-examine their plans for further development, it is well to have a look at the happenings in our discipline and to give a thought to its likely future developments. If one does this, and takes into account the symptomatic developments in Wolverhampton and elsewhere, it is difficult to avoid the conclusion, and here I perfectly agree with Professor Kendall, that the novel problems of statistics will be those suggested by progress in the applied fields. The second equally uncontestable conclusion is that, in order to be able to deal with these novel problems, the statistician of the future will have to be much better equipped mathematically than we oldsters are. Hazarding the regrets of some of the members of my generation, I would venture to assert that the future of statistics will be in the hands of those present young men, "embracing women", who, during their university days, manage to acquire mastery not only of the theory of measure but also of a host of other mathematical disciplines such as topology, modern algebra, Hilbert spaces, etc. The remainder of our students, who will be less well prepared, will not be on the forefront and will be reduced to something like routine activities. It is true that this routine work need not make them any less happy and that they always will be able to imitate us in dismissing the more advanced work with the sacramental formula "oh, but this is not really statistics, this is not a practical problem". But then a Boulton Paul Superairship Co. Ltd. is likely to appear on the scene and cause embarrassment.

Considerations of this kind suggest that if there is anything in the pre-war statistical instruction practice that has its days numbered then it is "mathematics for statisticians". Similarly, the days of "mathematics for physicists" are gone. The statisticians of the future, that is the statisticians capable of developing theory on a level adequate to deal successfully with the problems that are already on our horizon, will have to have in their tool box not "mathematics for statisticians" but "mathematics" without any adjective. Thus, when making plans for a statistical research and instruction unit, it is well to make

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provisions so that those of our students who show adequate promise and interest have the opportunity to learn this mathematics.

This is not so easy to arrange. I am in perfect agreement with Professor Kendall that statistics is an independent discipline, as are physics, etc., and requires independent departments in the universities. At the same time, just like modern physics, statistics requires contacts with pure mathematics and these, depending upon the structure of the particular university, are not always available. Even if they are available, teaching plans are difficult to organize, the time element is involved, a terrible nuisance! Still, the problem has to be faced.

(*f*) This brings me to my last point, one of regret. This regret concerns the status of the Statistical Laboratory, Cambridge University, of which I am currently a guest. Whether the late Director of the Laboratory, Dr. Wishart, wanted it or not, the Laboratory was one of the few statistical centres in which students had a good opportunity of learning modern mathematics. Undoubtedly, this was due to the excellent mathematical atmosphere in Cambridge and to the arrangement of the instruction programmes. Also, the supervision system made it necessary for the Laboratory's staff to be on their tiptoes in mathematical subjects. As things stand now, I frequently hear, through the wall of my room in the Laboratory, discussions of random variables followed by those of modern algebra, etc. As a result, the Laboratory produced several statisticians enjoying excellent mathematical preparation who now hold important professional positions.

With this distinguished status of the Cambridge Statistical Laboratory in mind and taking into consideration the outlook into the likely future developments of our discipline I find it unfortunate that, currently, the Laboratory appears to disintegrate: during the last few years it has lost four of its distinguished members and is in the process of losing a fifth. Out of the five losses, one was through the untimely death of Dr. Wishart. Two other members of the staff left in order to become Professors elsewhere. Another became a Reader in London and still another accepted an Associate Professorship in an American University. Unfortunately, no replacements are in sight and the programme of instruction in statistics and probability must be converging to zero. One may hope that, perhaps as a result of an appeal for public support of statistics implied by the present paper of Professor Kendall, this unfortunate trend will soon be reversed.

It is a great pleasure to thank Professor Kendall for his stimulating paper.

Dr. DAVID: I find myself in general agreement with much of what Professor Kendall has said, but I should like to comment on one or two points of detail. University College is a multi-faculty college and our procedure may be different from colleges with a single faculty. We are certainly organized internally a little differently. It is true to say that nearly all university appointments are to teaching posts, and little attention is paid to a person's teaching ability before he is appointed to such a post. Judgment is usually swayed almost entirely by research experience and publications. This is true with us in the Faculty of Science and holds in other faculties to a greater or lesser degree. Once in a teaching post promotion depends almost entirely on published work. We are therefore in the position which Professor Kendall envisages as something to aim at; we are research workers who do some teaching if for no other reason than that it is expected of us.

I am firmly of opinion that all research workers must be teachers and all teachers should do research. Only in this way can stagnation of thought by the teacher and sterility of ideas on the part of the research worker be avoided. The great mathematician, G. H. Hardy, is reported to have said that whenever he was short of a solution he could go to the cinema and let his subconscious mind come up with the answer. I find that the switch of concentration and thought necessary to give a lecture and the consequent irritability it induces are substitutes for cinema going, and certainly a refreshment which one cannot easily do without.

I feel that nothing can take the place of the Ph.D. as a training for research, and here,

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perhaps, Professor Kendall is a little hard on the post-graduate student. It is true that most research post-graduates have to have projects found for them but no supervisor, I think, to see if it is possible. The student is then alternatively ignored and pushed until he completes his problem. This process is fairly easy for an experienced supervisor but it is a training which cannot be given by a group. The chief element is teaching the student not to be afraid of tackling difficulties and if necessary making a fool of himself and this is best done by one person with a conveniently short memory.

It is perhaps surprising that Professor Kendall thinks, or I take it he thinks, that research is best done in a group. It is true that little research is done in isolation since it is vital to have colleagues who not only know statistics but who can pick up the thread of one's argument quickly so that difficulties can be discussed. The ideal, I think, is the association of a number of research teachers all of whom are interested in different aspects of statistics so that there is no overlapping of problems or of interest. In this way one is able to keep abreast of much of the research work which is going on and this is how we try to arrange things at University College.

Mr. HEALY: As Dr. Yates is abroad, perhaps I might make a few comments on behalf of the Rothamsted Statistics Department, as representing those research workers whose alternative occupation is not teaching but consultative and advisory work. There are many aspects in which our outlook is similar to Professor Kendall's. For example, we have tended to develop a nucleus of permanent staff at Rothamsted, while other workers have come to us for a year or two and then gone to work at other research institutes, returning at intervals for joint discussion of problems of mutual interest. We would also agree that the day of the individual research worker is over—at least in the sense that the essential tools of research, text-books, tables, calculating machines and so on, are simply too expensive for any individual to be adequately equipped. This in no way implies that all research must be team-work, but simply that it must be geographically centralized into groups large enough to provide for proper facilities and ancillary staff.

The chief point I should like to make is one to which Professor Kendall has perhaps given insufficient emphasis. He has been mainly concerned with the amount of research being done; I want to consider the directions it should take. For all that has been said about the self-sufficiency of statistics as a subject of study, we must admit that it is essentially an applied subject. Unless it is studied as a branch of pure mathematics, statistical research must be carried on with some field of application in view and if the research is going to be of value, the statistician has to know a good deal about this field. Professor Bradford Hill has often made this point about medical statistics and it is just as important in agriculture and biology. It is here that a department such as ours is in a very strong position. Surrounded as we are by biologists of many kinds, all demanding immediate solutions to important problems, we are seldom at a loss for new lines of research requiring investigation, or for guidance on the biological implications of our findings.

On the practical side, then, I would suggest that not only should teachers of statistics play a greater part in research, but that research workers in departments such as that at Rothamsted should do more teaching. We have, in fact, collaborated recently with the London School of Economics in a very successful course on sampling methods for colonial students, and I hope this new policy will be extended in many directions.

On a less important topic, I would register my disagreement with previous speakers who find that a Ph.D. is an essential part of a research worker's training. Most of our scientific staff have an honours degree in mathematics with a year's post-graduate work for a statistics diploma, and we find the additional time spent in acquiring a higher degree to be of doubtful value in many cases.

Mr. A. J. MAYNE: While I agree with much that has been said this evening, I feel that there is an important omission in the proposals so far made. Special provision needs to be made for the scientist who has ability and interest in fundamental research but little

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aptitude for teaching, though perhaps considerable skill at applied research. The statistical research unit at a university would not provide a really satisfactory environment for such a statistician, nor would a specialized research establishment, such as Rothamsted, nor an applied statistics post in public service, industry, or commerce.

I therefore propose that a new type of research establishment should be set up. Perhaps it should be a statistical, or a mathematical and statistical institute for advanced studies, not unlike the Institute for Advanced Studies at Princeton, U.S.A. The research staff of such an institute should be allowed to devote at least six months a year to fundamental research *of their own choice*, whether in new statistical techniques, or in new mathematical tools, or in scientific applications of statistics, or *in any combination of these*. They could devote the rest of their working year to specific applied research projects of practical importance. Alternatively, they could devote it to the writing of expositions and surveys of their fields of scientific interest.

The advantage of this new type of research unit is that adequate facilities could at last be given to the fundamental researcher who is temperamentally not fitted for teaching work, and would thus not be too happy in a university post. (I should point out here my strong disagreement with the views of Dr. David and Mr. Healy that *all* researchers should do some teaching.) The fundamental researcher could be given scope and time to develop his ideas in as many directions as his ability and versatility allow, and at the same time be given insight into and experience of the research needs of everyday life by his spells of practical research.

Such an institute should *not* be specialized. Worthwhile research would then be enabled to proceed in many and varied branches of pure and applied statistics, and perhaps also of mathematics. Individual researchers of the institute staff would thereby have a reasonable chance to do justice to *all* their important scientific interests and abilities. (The great achievements of the late Dr. von Neumann show that, even to-day, it is sometimes possible for a scientist to do outstanding research work in widely varied fields of science.) There would also be a valuable interchange of ideas between experts on widely varied fields of knowledge.

I would add the recommendation that, at such an institute, holidays (as well as other conditions of work) should be comparable to those obtainable in university posts. I believe that the present practice, of most public and private employers, of allowing excessively short holidays to their research staff has resulted in overworking them, often with serious damage to the quality and quantity of their research work.

Regarding Professor Kendall's remarks on training for research, I would draw attention to Oxford University's chemistry course, where a first degree is obtained on performance in one year's research as well as in examinations. It would be worth while trying a similar method for a first degree course in statistics.

Finally, I will consider the problem of communicating the results of statistical research to other statisticians and to the outside world. The first obstacle is the long time between the submission of a paper to a statistical journal and its publication there. The second obstacle is sheer lack of time by the researcher to write up his work. This can sometimes lead to delays of five years or more before an important piece of work appears in print. I would therefore recommend that statisticians prepare about a hundred duplicated copies each of provisional reports of their work, which would be either insufficiently developed or too detailed and specialized to be suitable for journal publication. They should circulate these reports to those other statisticians likely to be interested, and they should send at least one copy of each report to the library of the Royal Statistical Society.

Professor BARNARD: I feel I must add a further point of criticism,—that is, that we should not accept as trite the statement that more research into statistics is necessary. Surely, to justify any argument for more research into statistics, we need to specify the lines along which research should proceed, to indicate what results might be expected to accrue, and to show that the forces are available for the successful carrying through of such research.

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There is also one specific point of fact which needs correction. In para. 34 Professor Kendall states that no commercial firm "does what Guinness did fifty years ago in sending Gossett to University College". In fact we have the example of George Box, whose career was in some ways comparable with Gossett's, and the company who were responsible for him have now announced generally that they are prepared to do the same sort of thing again for other people with the same qualifications. Guinness in fact have themselves continued to send students to University College, and are by no means alone in so doing. It is true there are many other commercial firms which are not so far-sighted as this, but I think we ought not to let these good examples pass unnoticed.

Mr. W. BENJAMIN: I should like to make a plea for the industrial statistician. We have heard much about research groups at the universities and their methods of working and I should be interested to hear their reactions to the idea expressed by Mr. Healy who said that at Rothamsted lines of research were often decided by topics given them by other departments. How would the universities react to the suggestion that a section of their research group be given over to studying topics raised by industry?

Industrial statisticians usually work on their own or are attached to a (non-statistical) research group working on industrial problems. We come across problems where a new statistical technique would be of use but cannot usually spare the time to work one out. Often we are confronted with a problem to which we have to apply an orthodox statistical solution which we know is imperfect, because to provide a perfect or better solution would need far more time and possibly more mathematical knowledge than the industrial statistician could give it.

It would I think, be advantageous to both sides if for problems such as this there were recognized channels of communication between industry and university. I should be interested to hear from people in university or similar units how they would treat communications sent to them in this way.

Mr. D. V. LINDLEY: There is one matter that ought to be mentioned this evening and that is the equipment required of a person who is to do what Professor Kendall flatteringly calls "genuine" research. I refer not to the tools, such as calculating machines, that he requires, but to the preliminary training that he must have had before beginning research. It is no use being provided with elaborate tools if the ability to use them is not present.

In its early days statistics used comparatively simple mathematical tools. A little multi-dimensional geometry, perhaps, in distribution theory, but even this could be avoided with modest use of generating functions. Gradually the situation changed. A little group theory was needed in design of experiments. Statistics being inherently more complicated than probability theory, because it deals not just with one probability distribution but several, requires extensive results in measure theory and analytic topology. In probability theory even, use is now being made of semi-groups and the more highly developed properties of generating functions, or what are perhaps better called Laplace transforms. Consequently, it is now necessary for a research worker in statistics to be very highly trained mathematically. He does not have to have been trained in all these disciplines I have mentioned, but he must have the necessary background to read them up. This is also a reason for thinking that a statistics group should be part of the faculty of Mathematics. There is, I am aware, a view that such mathematical techniques are not needed in our subject and people who put forward these views will often adopt a superior attitude to at least one of the leading statistical journals. It can happen that a research worker produces by a laborious argument, using simple mathematical tools, a result which is a special case of an argument using more sophisticated methods that he is unable to understand and of which he is, therefore, suspicious. I do not suggest for a moment that all members of a unit should be qualified in the way I have proposed, but at least most of them should be.

This leads me to make one criticism of Professor Kendall when he remarks that every statistical paper should contain a practical example. That may have been true in the past, but ought not to be true in the future.

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The following contribution was received after the meeting:

Mr. GORDON NORTON: The effectiveness of practice in any branch of learning is related to the extent of knowledge of the practitioner. Where the branch is long-established and the area already well covered, the practitioner may be assumed to have acquired an adequate proportion of the available body of knowledge. Research in the branch is directed to extending the knowledge, and the larger the area the more specialized the research may be expected to be.

In directing research it is important, in terms of practising, to consider the relation of the direction of research to the initial area. There is currently a lot of activity in applying mathematical and statistical method to problems of various kinds, particularly in industry. The experience of this work is creating its own branch of learning and, as yet, the areas are small. Unless this experience is immediately related to directed research, the effectiveness of the work would itself be proportionately small.

In a small group of graduate scientists and mathematicians working in this activity, the policy on research has been firmly established. This group is independent, and has to find its revenue from fees in practising in industry, and the policy has been to establish the fees at such a level as will allow 25 per cent. of the time of the graduate analysts concerned to be spent in directed research. Initially, this 25 per cent. has been absorbed in covering the existing known area, but already it has been possible to develop the knowledge by research, and its value in being related to experience in practice has been very considerable.

It is notable that the class of honours graduate attracted to this work, that is to say, the best individuals that can be found available with first class or near first class honours, are themselves the type of person to whom research has a strong appeal. The net result is that the body of knowledge is being continuously extended, that the standard of practice is being constantly improved, and the members of the group derive progressive satisfaction, not only in relating their research to practice, but always knowing that ahead of every task there lies the attraction of original work.

Administratively it is going to need a strong hand and an unwavering determination to maintain this policy, but, being an independent group working only to specific tasks, it is possible to control the time of the individuals more effectively than would be possible in a group linked closely to activities in a single firm or organization, with the constant pressure of tasks known to the executive management.

Professor KENDALL subsequently replied in writing as follows:

The discussion was, I think, valuable and I am indebted to the contributors to it. For brevity I confine my reply to major points of disagreement or misunderstanding.

My remarks about the solo worker were misunderstood by some. I think that the solo *thinker* is still an important part of research. But, as Mr. Healy in particular appreciated, he is severely handicapped unless he has access to assistance such as a research unit can provide. I would agree that fundamental ideas often grow in a single brain and that creative thought is not necessarily or even frequently a team activity. But a good deal of statistical research involves much more than merely excogitating bright ideas.

I gladly accept Professor Barnard's correction about the broadmindedness of certain industrial concerns in sending men to universities for training. I hope, as I am sure we all do, that this will develop.

Mr. Mayne, I imagine, had in mind something like an institute for advanced study. There is a lot to be said for and against such a thing in this country. I myself am a little sceptical about ivory towers, but only to the extent that I place the onus of making the case on proponents like Mr. Mayne. Likewise I am doubtful about his suggestions concerning semi-publication. This, as we have seen, can lead to some very slapdash drafting and incomplete presentation.

I wholeheartedly disagree with Mr. Lindley about a statistics group necessarily being part of the faculty of mathematics. That some groups might usefully be so I will not deny. That they all should seems to me dangerous to our subject. One might equally

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well argue that physics, astronomy and engineering should be part of the mathematical faculty.

Mr. Fieller misquotes me in one respect. I did not say that university teachers are appointed only to teach. It is common to all appointments, I think, that the holder of a post is expected to advance his subject by research and other means. My point was that teaching requirements take priority, especially in times of financial stringency.

If I pass over the contributions from Professor Bartlett, Mr. Burchardt, Professor Neyman, Mr. Norton and the others it is not from want of appreciation but only that I think we are in broad agreement.

As a result of the ballot taken during the meeting the candidates named below were elected Fellows of the Society:

Michael Barratt Brown
Haldane Gladstone Coore
Eric Hugh Cownie
John Rodney Crossley
James Frederick Fry
Paul Galambos
Leo Goldstone
Joseph William Grant
Terence Edwin Hicks
Susumu Kikuchi
Louis Cunningham Swift Mansfield
Ivan Melinek
Bryan Ralph Morris
Joseph Pedoe
Eric Hardiman Mockford Price
Hugh Griffith Price
William Edward Prior
Harcharan Singh Randhawa
Celia Mary Roberts
John Macdonald Bennett Stocks
Gregory Ugalde
Joyce Margaret Weir
Richard Choi Kwong Yau

Corporate representatives

Peter Gorton Green *representing* Gillette Industries Ltd.
Grace Mary Jones, *representing* the Ministry of Pensions and National Service.
C. M. H. Morojele *representing* the Department of Agriculture, Basutoland.

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Differential Prices for the Services of Ships Belonging to the Same General Class—A Case of Oligopoly

By D. E. Kopanitsas

1. In economic theory, and even more so in applied economics, one has to make allowance for the fact that the physical units with which one deals, whether ships or any other commodities, are very rarely homogeneous. Although the degree of heterogeneity could be measured statistically, the economist has to take a practical decision on two issues. First, what physical qualities should he take as relevant in order to describe one commodity as differing from others in kind and then (after he has decided on such qualitative characteristics) what is the maximum degree of heterogeneity he should tolerate in an aggregate of commodities in order that he may treat them as homogeneous for the purpose of his study. Decisions like these, though unavoidable, suffer from the fact that they tend to be arbitrary, being a kind of value-judgments of the theorizing economist. From the point of view of economic relations, objectively considered, they can be significant only in so far as they refer to characteristics and magnitudes which are relevant in the market. I thought it might be useful if I studied a case where business men themselves had to decide on questions of heterogeneity. They have to do this for the purpose of attributing appropriate supply-prices to commodities which belong to the same general class (and therefore can be used as substitutes for each other) but differ in respect of some of the characteristics they possess.

2. A case with which I happen to be familiar is that of a Conference of ocean going passenger liners where the relative levels of the minimum prices, at which the services of the ships belonging to the member lines were allowed to be sold to the public, were fixed on the basis of differences in the age, size and speed of those ships. As the member lines had been unable to agree on a general formula giving specific weights to the three rating factors, the minimum fares to be allowed for a new ship entering the Conference had to be arrived at through negotiations and bargaining between the lines for that particular ship, no other arguments being in principle allowed in the discussions than those referring to the three rating factors. During the negotiations, each line, aiming at a competitive advantage, was trying to keep the minimum fares for its own ships as low as possible, the other lines, on their side, trying to force them to as high a floor as they could persuade the owners of the ship to accept. There were provisions in the Conference rules according to which, failing agreement, the fares would be fixed by arbitrators. The element of free consent was preserved through a provision allowing the member lines to leave the Conference by giving appropriate notice. The unwillingness of the lines to have the Conference broken up and see themselves isolated outside it, or their competitors free to act as they pleased without any of the checks the Conference was supposed to provide, has been an inducement towards moderation, so that the Conference has been going on existing and applying the fares fixed through its machinery. (The time to which the present information refers is the early part of the year 1956.)

Formally, each line has been free to change its fares by giving notice, but as a unilateral reduction could be expected to lead to similar action on the part of other lines, resulting

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in a fall of the general level of fares without a corresponding improvement in the competitive position of the line that initiated the movement, such notices, in the rare cases when they were given, were usually followed by negotiations or arbitrations leading to a new structure of fares, acceptable to all members.

It can be assumed (subject to verification) that the general object of the oligopolistic activity within the Conference has been to create a situation where differences in quality or kind of the ship services sold to the public (in so far as they are connected with differences in age, size and speed of the ships) were to be offset by corresponding price differences. The result is that instead of one homogeneous commodity we have a set of price/commodity relationships, which in theory could be described as a system of indifference curves. As this is a difficult object to achieve, the participants in this regulated type of competition have been anxious: (*a*) to detect the physical characteristics that are of greater importance to the public and arrange the price differences between the heterogeneous commodities in proportion to the measurable quantity in which such characteristics are possessed by each commodity; (*b*) to reduce the officially relevant characteristics to the minimum possible number, in order that negotiations and agreements on relative prices may be facilitated; and (*c*) to rule out certain other qualities which, if existent, might improve the competitive position of one ship to the detriment of others, while at the same time they could not be easily controlled, measured and rated.

The interesting point of this particular case of price regulation is that it gives us the opportunity of studying, in a concrete way, an attempt to overcome the practical difficulties connected with the co-existence of, and competition between heterogeneous commodities. The business men concerned have tried to achieve the rational and clear-cut object of co-ordinating the prices of heterogeneous commodities by the empirical and not always rational means of bargaining from point to point.

3. By applying statistical methods I have tried to find out how far the Conference lines have achieved their general object; whether the factors of age, size and speed, officially accepted by the Conference as the mainly relevant and decisive for this purpose, have really been reflected in the relative fares agreed upon and, if so, to what extent.

The questions involved can be stated as follows:

(*a*) To find an expression in terms of the free factors, which will fit the actual fares as closely as possible. (In statistical language this can be described as minimizing the mean square differences between theoretical and actual fares. The advertised minimum fares are taken as the dependent variable, whereas age, size and speed are the three independent variables, in terms of which the value of the dependent variable will have to be explained. To what degree this expression can explain the actual facts will be shown by the multiple regression coefficient.)

(*c*) To estimate by how many dollars the advertised fares of a ship would on the whole tend to become higher or lower, according to whether the given ship would be by one year newer or older, by a thousand tons larger or smaller, or by one knot faster or slower. This is to be estimated for each factor separately, with due allowance for the contribution of the other two factors. (This is done by calculating the partial coefficients of regression of each one of the factors with respect to the minimum fares.)

4. The age (or rather, the year in which the building of the ship was completed), the size and the speed, on which this enquiry has been based, are those checked by the Conference and allowed to be advertised.

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The fares are the minimum for tourist class accommodation between the same ports, on two-class ships belonging to the Conference, as advertised at the beginning of 1956 for the "low season" of the same year, namely for the season when the demand by the public for accommodation is at its lowest and, hence, competition between the lines at its highest.

The fares actually advertised are not necessarily as low as the member lines are allowed to quote. The lines are free, if they so wish, to quote and sell accommodation at a higher price. Moreover, the actual speed, at which the service is performed and which the lines are allowed to advertise, may differ from the speed that was originally taken into account by the Conference when the minimum fares were fixed. Age too can have an ambiguous meaning, considering that several of the ships are older vessels converted and modernized. Consequently, the fares, the speed and the age, on which the present calculations are based, are not invariably those originally taken into account by the Conference when rating the ships. While the original pattern of relations between physical characteristics and relative prices may have been distorted, the new pattern resulting from the change can be equally significant. The fact that the Conference records and tolerates the changed conditions means that it regards the new combination of fares and physical characteristics as good enough to prevent appreciable competitive advantages of some of the ships over the rest.

This kind of equilibrium is, of course, an ideal object; a reasonable indication that, in the view of the interested parties, it has been approached to a tolerable approximation emerges from the fact that the system of the minimum fares advertised has met with the approval of the Conference.

The present statistical exercise can have no other purpose or excuse than the wish to ascertain whether, and how far, these relations could be brought within the scope of some simple and generalized formula. The concrete values of the parameters of the formula to be found are a matter of secondary importance.

The variables used in this connection are shown in Table 1.

5. (A) The general equation (regression equation) giving the nearest approximation to the minimum advertised rates in terms of age, size ond speed, has been found to be:

$$x_1 = 0{\cdot}2415x_2 + 0{\cdot}9649x_3 + 0{\cdot}2386x_4, \text{ where}$$

x_1 stands for the difference between the advertised minimum fare of a ship belonging to the group and the average of such fares for all ships of the group;
x_2 for the difference between the year the ship was built and the average for all ships belonging to the group;
x_3 for the difference (in thousands of tons) between the gross tonnage of a given ship and the average for all ships;
x_4 for the difference between the speed of a given ship in knots and the average for all ships, in 1954–55.

How far this equation goes to explain the advertised minimum fares is shown by the square of the multiple correlation coefficient, $R^2_{1(234)}$, the value of which has been found to be equal to 0·84458, while the part of the fares that remains unexplained by the above equation is denoted by the standard deviation or error of the third order

$$s_{1\cdot234} = 3{\cdot}5333$$

In ordinary language, this, more or less, means that the advertised minimum fares are explained by the regression equation up to about 84 per cent., the remainder, which is left

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TABLE 1

Basic Characteristics and Minimum Rates for Tourist Class During the "Low Season", between the Same Ports, of Two-class Ships Belonging to the Same Conference of Passenger Ships (at the beginning of 1956)

Serial Number of Ship	*Advertized Minimum Tourist Class Rate, Low Season (Early 1956) in $s* X_1	*Year when the Building of the Ship was Completed* X_2	*Gross Tonnage of the Ship* X_3	*Actual Service Speed in Knots, (Average for the Period 1954–55) or (in Brackets) Estimated Speed* X_4
1	172	1955	26,000	20·00
2	170	1955	21,717	(19·00)
3	170	1931	26,000	20·00
4	170	1954	21,637	(19·00)
5	165	1930	27,666	16·47
6	165	1952	20,459	22·02
7	165	1952	15,024	16·60
8	165	1951	15,014	16·60
9	162	1930	20,100	(15·00)
10	161	1930	26,313	19·59
11	161	1928	21,663	15·53
12	157	1928	20,448	17·15
13	157	1940	11,733	15·14
14	157	1930	12,150	14·89
15	157	1922	16,991	14·61
16	155	1923	20,341	14·48
17	155	1920	19,930	15·35
18	145	1920	10,474	14·37
19	145	1925	14,440	13·96
20	145	1913	9,424	14·29
21	145	1920	7,572	13·85
22	145	1947	7,438	14·66
23	145	1948	7,437	14·42
24	145	1931	7,114	14·03

unexplained by the equation, corresponding to approximately $3·53, on the average. However, the three or four dollars which are not accounted for by the official rating factors can be of great practical importance to the Conference lines, as the toughest negotiations very often centre around differences in fares not greater than one or two dollars.

(B) How much each of the three factors taken separately is correlated with the advertised minimum fares is shown by the corresponding partial correlation coefficient (the calculation of which implies that due allowance was made for the contribution of the other two factors to the joint determination of the rates).

Correlation between Minimum Advertised Fares and:	*Partial Correlation Coefficient*
Age	$r_{12·34} = 0·572$
Size	$r_{13·24} = 0·761$
Speed	$r_{14·23} = 0·086$

In order of magnitude, the partial correlation of fares with size is the highest of the three. Next comes the correlation of fares with age. The correlation of fares with speed is much below the others and, moreover, cannot be regarded as significant. On the contrary, the first two must be accepted as significant because the chances that there is no correlation at all and that their values as calculated are due to an accidental combination of the figures compared, are less than 1 per cent. (The level of significance has been

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tested with the help of R. A. Fisher's tables and by using degrees of freedom equal to the number of pairs less 4, i.e. 20; see R. A. Fisher (1954), *Statistical Methods for Research Workers*, 12th ed. Edn. Oliver & Boyd. Table and note on p. 209, and p. 196.)

It is hardly surprising that there is no significant correlation between fares and recorded speeds, considering, first, that if such a correlation were originally intended it may have been subsequently upset by differences between the speeds taken for rating purposes and the speeds actually achieved during the period 1954–55, and, secondly, that differences in speed may have been ignored by the interested lines if they happened to be not sufficiently large as to affect the number of days required for the voyage. Similarly, the low correlation between fares and age may be connected with the difficulty of ascribing a definite age to renovated vessels.

The high coefficient of partial correlation between size and fares can be easily attributed to the additional conveniences made possible by greater size (for instance, larger deck space and public rooms), to the prestige and snob value of a larger ship and to the fact that size is a fairly precise and stable characteristic about which there can probably be less ambiguity than about the other rating factors.

There is a serious complication arising from the possibility that what we take as independent variables may, in fact, be inter-connected. That this is probably the case is roughly indicated by the coefficients of zero order correlation of each of the variables age, size and speed, to each one of the other two:

Zero Order Correlation between:	
Age and size . . .	$r_{23} = 0 \cdot 2290$
Age and speed . . .	$r_{24} = 0 \cdot 5873$
Size and speed . . .	$r_{34} = 0 \cdot 7168$

The correlations of speed to age and size are significantly high and, therefore, it becomes logically difficult and unsafe to grade the relative importance of these factors with reference to the determination of the minimum fares, considering that the determining factors are not clearly independent of each other. It may be that the partial correlation coefficients of the second order $r_{12 \cdot 34}$ and $r_{13 \cdot 24}$ have over-estimated the importance of age and size as rating factors, at the expense of speed. For this reason it has been deemed advisable to keep the factor speed in our system of equations and also to show it in the regression equation.

(c) From the regression equation

$$x_1 = 0 \cdot 2415x_2 + 0 \cdot 9649x_3 + 0 \cdot 2386x_4$$

and the values of the partial regression coefficients, we can see that, other things remaining equal,

(*a*) ships older by one year tend on the whole to be associated with fares lower by \$0·24;

(*b*) ships larger by 1000 tons gross tend on the whole to be associated with fares higher by \$0·96;

(*c*) ships faster by one knot tend on the whole to be associated with fares higher by \$0·24.

From Table 1 it can be seen that the variables used for the present purpose are not polarized into groups of distinctly high and distinctly low values but are more or less

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TABLE 2

Testing the Significance of the Coefficients of Regression of the 2nd Order

(*Note:* The standard errors of the coefficient of regression of the 2nd order are calculated in accordance with a formula of the type

$$S_{C12\cdot k} = \frac{S_{1\cdot 2k}}{S_{2\cdot k}\sqrt{n}},$$

where n is the number in the sample and k a collection of secondary subscripts other than 1 and 2; Yule and Kendall, *An Introduction to the Theory of Statistics*, 19.26.)

Regression Coefficients of the 2nd Order	$S_{1\cdot 2k}$	$S_{2\cdot k}$ *etc.*	$\sqrt{n}$	$S_{2\cdot k}\sqrt{n}$ etc.	$S_{C12\cdot k} = \frac{S_{12\cdot k}}{S_{2\cdot k}\sqrt{n}}$ *etc.*	$t = \frac{C_{12\cdot k}}{S_{C12\cdot k}}$ *etc.*	*Significance of the Regression Coefficient of the 2nd Order (as shown by t, for $n-1-p=20$ degrees of freedom; Kendall,* The Advanced Theory of Statistics, 22, 31)
$C_{12\cdot 34}=0\cdot 2415$	$S_{1\cdot 234}=3\cdot 5333$	$S_{2\cdot 34}=10\cdot 2050$	$\sqrt{24}=4\cdot 899$	$S_{2\cdot 34}\times 4\cdot 899=49\cdot 9943$	$S_{C12\cdot 34}=0\cdot 0707$	3·416	Highly significant.
$C_{13\cdot 24}=0\cdot 9649$		$S_{3\cdot 24}=4\cdot 2653$		$S_{3\cdot 24}\times 4\cdot 899=20\cdot 8957$	$S_{C13\cdot 24}=0\cdot 1691$	5·706	,, ,,
$C_{14\cdot 23}=0\cdot 2386$		$S_{4\cdot 23}=1\cdot 2702$		$S_{4\cdot 23}\times 4\cdot 899=6\cdot 2227$	$S_{C14\cdot 23}=0\cdot 5678$	0·420	Non-significant.

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evenly graded. They do not even possess individual values that could be described as exceptionally high or low. Such outliers could have rendered the regression coefficients meaningless. In order to achieve this effect, it has not been necessary to have recourse to intentional pruning. It so happened that by confining the enquiry to two-class ships, outliers (e.g. very large ships, which it so happens are mostly operated as three-class carriers) were automatically excluded.

The tendencies indicated by the partial regression coefficients are, of course, subject to deviations from case to case. How close the deviations keep to the respective general tendency and, consequently, how far such a general tendency could be regarded as significant, can be estimated by comparing the general tendency (coefficient of regression) to the standard error of the corresponding coefficient of regression.

Table 2, where such calculations have been carried out, shows that the coefficients of partial regression of fares on age and size are highly significant, whereas that of the regression on speed is not (the value of the regression coefficient being less than half of its standard error).

The differences of the actual minimum fares from the corresponding theoretical values, as calculated in accordance with the general regression equation are given in Table 3.

TABLE 3

Comparison of Actual Fares to those Calculated in Accordance with the General Regression Equation

Serial Number of Ship	*Actual Fares* (1) $	*Calculated Fares* (2) $	*Deviation of Actual from Theoretical Fares* (*col.* 1–*col.* 2) (3) $
1	172	172·05	−0·05
2	170	168·15	+1·85
3	170	166·50	+3·50
4	170	167·60	+2·40
5	165	167·03	−2·03
6	165	166·70	−1·70
7	165	160·17	+4·83
8	165	159·92	+5·08
9	162	159·37	+2·63
10	161	166·47	−5·47
11	161	160·52	+0·48
12	157	159·73	−2·73
13	157	153·74	+3·26
14	157	151·67	+5·33
15	157	154·34	+2·66
16	155	157·79	−2·79
17	155	156·87	−1·87
18	145	147·52	−2·52
19	145	152·45	−7·45
20	145	144·79	+0·21
21	145	144·59	+0·41
22	145	151·18	−6·18
23	145	151·36	−6·36
24	145	146·85	−1·85

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The Efficiency of Randomization by Card Shuffling

By D. D. Kosambi and U. V. Ramamohana Rao

This paper compares the theoretical expectations with the results actually observed in mechanical shuffling of 25 cards divided into 5 suits of 5 denominations each. The results may be easily extended to m suits of n denominations each. The specific choice of 5×5 was made in order to consider the validity of some results announced by experimenters in parapsychology and extra-sensory perception (*ESP*). The basic assumption on which the statistical analysis would have been valid, namely, that the material (cards) has been efficiently randomized by shuffling, is not supported by our trials.

Some workers (Feller, 1940; Rhine, 1950, 1954, 1956*a*, *b*; Soal and Bateman, 1954; Soal, 1956; Spencer Brown, 1953, 1956; Tyrrell, 1954; Boring, 1955; Bridgman, 1956; Price, 1955, 1956; Meehl and Scriven, 1956) speak of a given sequence of cards being a random sequence or not a random sequence. This is meaningless from any statistical point of view. The "randomness" can only be attributed to a process of selecting a set of sequences; it can be tested only by considering whether the ordering of the cards can be a random sample from a population with some previously accepted distribution. The assumption which appears in all text-books on the subject is that sequences of shuffled cards are random, in the sense that one may assign an equal probability to every possible order of the cards. It has been further noted on this assumption that the cards are not dealt in random fashion in certain types of card games (Kendall, 1945, pp. 297–299, example 12.1). Here, however, the process of randomization consists not only of shuffling but also of the deal and play. In the present case, only shuffling in a long, closed shuffling box of the *ESP* model has been used. There was one added refinement which does not affect in any way the randomness or otherwise of the 25 cards. This consisted of the addition of a 26th "marker" card, the joker. Thus, the pack shuffled consisted of the joker and the 5 suits: C(lubs), D(iamonds), H(earts), S(pades) and T(rumps), in each of which there were the denominations: A, K, Q, J and 10. The shuffled sequence was always taken to begin with the card after the joker, continued in cyclic order to the last card which was either the card at the bottom of the pack (when the joker was on top) or otherwise the card on top of the joker. The advantages of this will appear later, and the procedure amounts to shuffling with a cut, so that a mere cyclic rotation of the cards would have been discovered immediately—though no such appeared in the course of our observations. In recording, both suit and denomination were entered, as well as the actual position of the joker in the sequence, so that any other—say the natural—order of the cards could also be marked precisely. No essential difference is made to our final conclusions by taking the natural order instead of beginning with the card after the marker. Ordinary factory-made playing cards were used with a suitable convention as regards the fifth suit.

It is seen at once that shuffling, recording and analysing enough sequences so as to represent each of the 25! possible orders of the cards would be beyond the resources of any laboratory not equipped with special mechanical and electronic devices. A further question would be of the effect of wear upon the cards used in the process. It follows, therefore,

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that the test of randomness had to be applied to some function of the order, with known probability distribution. If the process of shuffling adopted shows significant deviation from the theoretical distribution for that particular function, it follows that the process of randomization is not efficient under the basic assumptions of the problem. It is not excluded that the shuffling may at the same time be efficiently random with regard to some other function of the sequence. The use of shuffling in experimental designs is, therefore, valid only if it has been proved that the type of shuffling used gives an observed distribution compatible with the theoretical distribution of the sequence function chosen. Even then, there may be runs in the observed sequences that deviate significantly from expectation; such runs have frequently been observed by us, when a somewhat larger run seemed to agree reasonably well with the expectation. So the size of the run to be used is also a factor that the experimenter should take into consideration. Finally, our work shows that the significance or otherwise of the randomization may depend upon the initial order.

When no distinction is made between two cards of the same suit (or denomination) the procedure matches that of *ESP* observations. Even so, the number of different sequences is (25!)/(5!) still far too great to be handled by our meagre resources, if each particular sequence that can be distinguished from every other were expected to occur about twenty times. The assumption is still that these sequences with suits (or denominations) confounded have a uniform distribution, though this need not simultaneously be true (for a given process of shuffling and a given initial order) of both the 25! and (25!)/(5!) sequences. With only 5 varieties in 25 cards, a "decimal" notation in the scale of 5 can be adopted whereby each sequence is represented by just one point on the real line segment $0 \leqslant x < 1$. But there are uneven gaps in the line where no sequence will lie, so that the distribution on the line is not uniform. That is to say the line segment could not be divided into a few convenient pieces of equal length with equal probability measure. The situation (and the *ESP* results) would be greatly improved if 25 cards were to be chosen at random from a large aggregate wherein each of the 5 suits occurred in equal numbers. Spearman's rank correlation suggests itself as a function of the sequences, which may be used for comparison with the initial order; and its distribution is known. The difficulty in using this is not only one of calculation but also of the ties that will be scored in certain special arrangements; and our initial arrangement, in particular, involves the maximum number of ties for suits, minimum for denominations. The same difficulty arises with Fisher's assignment of normal scores to ranked data. Fisher's scores would really not be observed in random selection from a normal variate, as the mean for every sequence of 25 is exactly zero. In shuffling, however, one usually expects a card to be moved away from its initial position and initial neighbours. The distance a card is shifted is, in some way, measured either by the rank correlation or by assigning scores, so that the shift or a suitable function of the displacement could be taken as a measure of the shuffling. The conjugate, namely the number of cards which remain in their original positions, could therefore also be used as a measure of the randomization. The *ESP* observations record the number of coincidences between each shuffled 5×5 sequence with a corresponding set of 25 guesses by the subject, not restricted to a 5×5 division. In our work, the "hits" (coincidences) were recorded between the shuffled sequence and the initial order fixed at the beginning of the whole run of sequences. It follows that the initial order could be any that we chose and the randomness of the process would not be affected *in theory* because this procedure amounts to performing the same permutation upon every recorded sequence.

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The distribution of the number of coincidences is in some doubt, if the *ESP* work be taken into account. The *ESP* observers maintain that the distribution of the "number of hits" is binomial, with $p = \frac{1}{5}$ and $q = \frac{4}{5}$. Their argument is that the subject tested is free to name any one of the 5 varieties for each card exposed. The subject is not shown the card nor given any sensory cue as to the nature of the cards exposed. The argument seems fallacious to us, because it is known on the basis of other experiments (Estes, 1954, Chap. 9) that human subjects tend to approach the correct frequency asymptotically even when they did not know it in advance. Though the *ESP* data on this are not available, we should expect that their subjects, knowing that there were 5 cards each of 5 varieties would on the whole tend (consciously or not) to guess each variety about one-fifth of the time for every sequence. However, this is not obligatory on their part. The subject who guessed the same variety for each of the 25 cards would then score 5 with unit probability, while the probability of any other score would be precisely zero. If the parent distribution were binomial the average number of hits would still be 5 but this will have a probability distribution, when the same suit is guessed 25 times in succession, which differs markedly from the 0, 1 distribution. For our purpose, where the coincidences are taken with some fixed sequence of these cards, the distribution certainly cannot be binomial. The probability of exactly 24 hits must, in our case, be zero, as the remaining card would then necessarily score a hit. It is easy to show on rather plausible assumptions that the distribution is as given in Table 1, which has been calculated on the basis of 5 independent stochastic variables added together, where each of the variables has equal probability of every one of its 5! arrangements. Again, it is not clear whether the same process of shuffling which will give

TABLE 1.

Frequency Distribution of Hits

Hits	*Frequency*
0	164,916,224
1	843,321,600
2	2,099,785,600
3	3,484,896,800
4	4,361,769,500
5	4,369,364,605
6	3,643,332,100
7	2,600,896,850
8	1,622,130,800
9	900,412,125
10	450,613,840
11	205,356,600
12	85,974,600
13	33,073,050
14	11,811,000
15	3,894,860
16	1,191,600
17	342,850
18	86,800
19	23,000
20	4,220
21	1,225
22	100
23	50
24	0
25	1
Total	$(5!)^5 = 24,883,200,000$

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a uniform distribution for the 25! or (25!)/(5!) cases would act uniformly in this sense. If, however, ours were nearer the correct distribution *ESP* workers would tend to observe a highly significant number of cases with 10 hits or more, depending upon the regular partition of the subject's guesses. This incompatibility is seen by column 4 of Table 8. The χ^2 value calculated on the basis of the binomial as the true distribution and ours as the observed, is decidedly higher ($\chi^2(9 \text{ d.f.}) = 66{\cdot}91^{***}$). Naturally, the χ^2 value would be proportional to the sample size; here $N = 2{,}400$.

In these circumstances, it is essential to make some test of randomization which is independent of these two parent distributions. The addition of the marker card enables us to do this precisely. Table 2 shows the actual distribution of the 26 possible positions

TABLE 2

Position of the Marker Card

	5 *Shuffles* (*Exp.* = 31)		10 *Shuffles* (*Exp.* = 31)		*Continued Shuffling* (*Exp.* = 38)	
Position	*Number of Trials*	χ^2	*Number of Trials*	χ^2	*Number of Trials*	χ^2
0	63	33·03***	69	46·58***	78	42·11***
1	35	0·52	32	0·03	39	0·03
2	27	0·52	33	0·13	33	0·66
3	32	0·03	21	3·23	36	0·11
4	25	1·16	20	3·90*	39	0·03
5	29	0·13	28	0·29	37	0·03
6	30	0·03	30	0.03	37	0·03
7	30	0·03	29	0·13	26	3·79
8	29	0·13	27	0·52	43	0·66
9	35	0·52	19	4·65*	34	0·42
10	37	1·16	20	3·90*	38	0·00
11	20	3·90*	34	0·29	34	0·42
12	37	1·16	29	0·13	28	2·63
13	39	2·06	27	0·52	35	0·24
14	27	0·52	29	0·13	58	10·53**
15	19	4·65*	39	2·06	25	4·45*
16	33	0·13	32	0·03	35	0·24
17	30	0·03	37	1·16	44	0·95
18	32	0·03	28	0·29	34	0·42
19	26	0·81	48	9·32**	40	0·11
20	28	0·29	22	2·61	29	2·13
21	27	0·52	34	0·29	37	0·03
22	34	0·29	28	0·29	38	0·00
23	28	0·29	30	0·03	51	4·45*
24	28	0·29	35	0·52	28	2·63
25	26	0·81	26	0·81	32	0·95
Total	806	53·04***	806	81·87***	988	78·05***

of the joker. The observations were made in three separate series, of which the first replaced all cards of the sequence in the initial order, after 5 tilts of the shuffling box. The joker was initially on top. The second series was with replacement after 10 tilts of the shuffling box. The final and the most important series was of continued shuffling without any replacement whatsoever; between two observations the box was tilted 5 times. Again, the number of 5 tilts was suggested by the *ESP* procedure. It will be seen at a glance that the joker does not occupy each of its positions with equal frequency. It was quite surprising to find that the joker tended to remain on top even for the sequence of 1,000 shuffles without replacement, each shuffle being always of 5 tilts of the box.

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Another such test is based upon the principle of block design. This amounts to asking whether each variety comes equally often next to every other variety. For any given random sequence, the probability of the card occurring in a given position depends upon the cards that have preceded in the sequence. That is to say we must deal here with a Markov chain. The full calculations may be neglected because the number of such observations is large, and random. However, the average probablity of a card being succeeded by another of the same variety is 4/24 whereas it is 5/24 for any other variety. Thus, we may arrange the cards in a 5 × 5 square array, where the row indicates the card preceding, and the column indicates the card following it. The initial order of the suits was C, D, H, S and T and within each suit A, K, Q, J and 10. The results are displayed in Tables 3 (*a*) and 3 (*b*).

TABLE 3

Continued Shuffling 1,000 *Trials*

Frequency of juxtapositions (Expected: CC etc. or AA etc. = 833·32
CD etc. or AK etc. = 1041·67)

(*a*) *Suits*

	C	D	H	S	T
C	950***	1,071	973*	974*	1,032
D	1,044	820	1,097	1,040	999
H	1,033	973*	975***	928***	1,091
S	990	1,181***	918***	873	1,038
T	983	955**	1,037	1,185***	840

χ^2(16 d.f.) = 142·54***

(*b*) *Denominations*

	A	K	Q	J	10
A	835	1,102	1,082	1,050	931***
K	1,065	831	1,004	1,081	1,019
Q	1,045	998	813	1,090	1,054
J	951**	1,030	1,065	771*	1,183***
10	1,104	1,039	1,036	1,008	813

χ^2(16 d.f.) = 63·20***

These show a significant deviation from expectation. The tabulation is exhibited only for the continued 1,000 shuffles without replacement.

To avoid the question of a Markov chain distribution, we considered merely the card after the joker. The results are displayed in Table 4, grouped in batches of 200 observations each for 5, 10 and the continued shuffling. It will be seen that in the first two cases the club suit which was initially adjacent to the joker retained that position far oftener than

TABLE 4

Suit after the Marker Card (200 *Trials in Each Batch*)

Variety / *Batch*	5 *Shuffles* C	D	H	S	T	10 *Shuffles* C	D	H	S	T	*Continued Shuffling* C	D	H	S	T
(i)	72	40	26	34	28	55	30	42	38	35	32	31	37	58	42
(ii)	95	23	31	25	26	64	33	39	29	35	24	47	26	58	45
(iii)	101	19	22	26	32	69	26	38	21	46	36	49	35	39	41
(iv)	81	28	30	33	28	66	28	32	39	35	38	33	22	45	62
(v)	72	30	33	34	31	56	43	39	36	26	34	58	35	51	22
Total	421	140	142	152	145	310	160	190	163	177	164	218	155	251	212

χ^2 (4 d.f.) = 305·67***
Clubs *v.* the rest:
χ^2 (1 d.f.) = 305·26***

χ^2 (4 d.f.) = 78·49***
Clubs *v.* the rest:
χ^2 (1 d.f.) = 75·63***

χ^2 (4 d.f.) = 31·95***

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would be expected on the hypothesis of efficient randomization. It is surprising to find that though this suit lost its position on continued shuffling, the distribution among the suits was still far from uniform. If the shuffling were efficiently random, each entry in the body of the table should be about 40 and in the totals column close to 200. It will be seen that in both cases of shuffling with replacement, practically all of χ^2 arises from the difference between the favoured suit (clubs) and the rest. The total χ^2 decreased when the number of tilts of the shuffling-box was doubled, while the share due to the favoured suit was relatively less.

These considerations would prove, without any further discussion, that card shuffling is a far less efficient method of randomization than has been assumed.

It remains only to comment on certain features of the data. Of these, the most interesting is perhaps that tabulated in Table 5 for continued shuffling. This shows the frequency with which every card occupied every position. The total value of χ^2 for this table, on the assumption of uniformity, is 733·20 for 576 d.f. which comes very close to $p = 0{\cdot}001$. In addition, it will be noted from the table that certain individual cards occupied special positions far too often. For example, S_K occurs in the position C_A, 70*** times and T_A in T_J, 61*** times. If the theoretical distribution of χ^2 be computed for the corresponding number of degrees of freedom, it will be noted that the values of χ^2 actually observed show a distribution significantly different. This is even more convincing than an individual case with one single value of χ^2.

The distribution of "hits" to be used as test of efficiency can be scored best against the initial order. In this case, it is seen that the suits being as close as possible and the denominations being as far apart as possible the number of hits should on *a priori* reasoning behave differently, if the order tends to persist. In effect Table 6 shows that the suit-hits are very highly significant for both the types of shuffling with replacement, whether the parent distribution be taken as binomial or the one we have given. It is further seen that the observed distribution of hits (suits) for 5 and 10 shuffles with replacement are not compatible. For continued shuffling, the observed frequencies of hits (suits) are significant at the 5 per cent. level for our distribution but not for the binomial. However, the case of 7 hits is, for some unknown reason, highly significant for our distribution and significant at 5 per cent. with the binomial. For denominations, Table 7 shows that the association with the original position continues only for the two modes of shuffling with replacement. There is no significance left for denomination hits, with continued shuffling, though even here some runs of about 200 were observed to behave in a significantly different manner from theoretical expectation.

The case for *ESP* is considerably weakened by these observations. As a point of comparison we may consider Pratt and Woodruff's data, Series B (1939), in which they give two sets of hit-frequencies whereof the first, according to the experimenters, proves the existence of *ESP*, because the subject was trying to guess the sequences exposed, while the second takes the number of hits scored by the same guess-sequence against the third run in advance of the particular trial. The latter was found by the experimenters to agree with the binomial distribution ($p = 1/5$) and taken as proof that their randomization is beyond reproach. We may point out that the results appear entirely different, if our theoretical distribution of Table 1 were to be taken as the basis. In that case, both their original and cross-check data would be very highly significant. It may further be remarked that for our data (1,000 continued shuffles), a fixed guess of the type S H H C D T S H C D T

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TABLE 5

Continued Shuffling (1000 *Trials*)

Position	C_A	C_K	C_Q	C_J	C_{10}	D_A	D_K	D_Q	D_J	D_{10}	H_A	H_K	H_Q	H_J	H_{10}	S_A	S_K	S_Q	S_J	S_{10}	T_A	T_K	T_Q	T_J	T_{10}
Card																									
C_A	.	27*	35	38	45	50	41	36	40	42	34	48	42	49	48	46	32	41	24*	38	35	41	48	36	32	52
C_K	.	36	40	29	50	31	39	36	38	35	41	26*	49	41	42	35	51	34	47	42	46	40	46	47	40	39
C_Q	.	22**	42	36	40	40	40	39	32	53*	30	48	32	43	43	47	55*	36	41	37	36	36	53*	37	39	43
C_J	.	35	43	36	43	49	39	34	29	42	40	41	39	43	46	35	43	53*	42	36	25*	38	43	41	25*	60**
C_{10}	.	44	35	35	43	34	44	46	41	48	50	42	32	38	39	34	35	45	47	44	45	34	31	37	45	25*
D_A	.	56*	44	36	38	55*	36	35	44	40	41	36	47	39	37	34	38	45	44	31	45	43	42	41	36	28
D_K	.	40	38	47	55*	43	40	50	46	37	44	34	39	39	41	37	36	53*	42	33	37	36	40	32	34	43
D_Q	.	34	57**	39	31	42	49	47	41	36	47	36	39	35	33	37	33	47	45	33	35	30	42	38	43	41
D_J	.	48	35	35	41	46	36	35	40	40	43	53*	43	36	22**	40	38	51	45	43	49	32	38	32	40	39
D_{10}	.	40	32	49	37	31	37	41	44	32	39	31	44	35	35	44	39	53*	40	47	33	50	40	54*	46	48
H_A	.	36	48	47	24*	37	31	38	36	30	36	46	29	40	43	40	44	39	47	43	40	53*	36	36	38	43
H_K	.	26*	43	41	50	47	45	49	47	43	42	36	29	38	43	43	32	30	43	43	40	44	40	38	39	26*
H_Q	.	37	47	34	44	45	40	40	47	46	40	30	40	35	47	46	37	47	35	34	34	26*	40	38	28	44
H_J	.	20**	43	39	40	46	37	28	48	41	33	35	44	42	55*	46	33	42	30	41	39	40	50	48	46	39
H_{10}	.	36	39	47	23**	40	36	42	40	36	48	44	45	36	39	46	33	40	47	37	48	38	38	41	40	45
S_A	.	55*	42	45	36	42	37	39	44	37	30	36	45	36	37	46	35	45	45	30	47	42	34	34	40	53*
S_K	.	70***	39	39	44	31	33	51	41	40	40	48	44	46	41	29	47	33	51	41	36	39	38	30	27*	22**
S_Q	.	48	29	45	40	33	40	39	40	37	31	34	39	40	37	45	42	27*	48	42	36	60**	37	42	44	45
S_J	.	36	38	28	31	36	32	43	35	50	51	40	47	40	56*	42	39	34	30	41	42	43	36	39	40	51
S_{10}	.	42	35	50	40	36	39	36	44	48	45	46	35	36	29	50	35	37	33	41	39	40	45	39	37	43
T_A	.	42	36	50	43	30	47	46	36	26*	35	52	45	46	44	26*	46	38	35	47	42	39	38	44	46	29
T_K	.	51	46	34	48	44	36	43	40	40	45	39	46	36	36	50	38	31	37	37	41	25*	41	33	51	32
T_Q	.	39	43	39	42	37	43	32	40	38	42	41	44	40	34	38	44	41	48	44	44	38	33	43	39	37
T_J	.	59**	37	41	38	36	51	35	42	37	35	36	38	34	30	39	55*	36	44	32	39	39	35	50	38	46
T_{10}	.	21**	34	41	34	39	52	40	25*	46	38	42	43	33	43	41	36	49	41	49	38	41	41	52	61***	27*

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TABLE 6

Distribution of Hits (Suits)

Hits	*Exp. No. of Trials*	*Bino Exp. No. of Trials*	χ^2_{AB}	5 *Shuffles*			10 *Shuffles*				*Continued Shuffling*		
				Obs. No. of Trials	χ^2_A	χ^2_B	*Obs. No. of Trials*	χ^2_A	χ^2_B	χ^2_C	*Obs. No. of Trials*	χ^2_A	χ^2_B
0 and 1	40·5	27·4	2·53	90	60·50***	143·02***	63	12·50***	46·25***	4·76*	35	0·75	2·11
2	84·4	70·9	1·17	86	0·03	3·22	73	1·54	0·06	1·06	79	0·35	0·93
3	140·1	135·8	0·07	104	9·30**	7·45**	111	6·04*	4·53*	0·23	143	0·06	0·38
4	175·3	186·7	0·36	126	13·86***	19·73***	160	1·34	3·82	4·04*	167	0·39	2·08
5	175·6	196·0	1·12	116	20·23***	32·65***	174	0·01	2·47	11·60***	183	0·31	0·86
6	146·4	163·3	0·92	115	6·73**	14·29***	132	1·42	6·00*	1·17	149	0·05	1·25
7	104·5	110·9	0·19	98	0·40	1·50	129	5·74*	2·95	4·23*	138	10·74**	6·62*
8	65·2	62·4	0·06	78	2·51	3·90*	65	0·00	0·11	1·18	55	1·60	0·88
9	36·2	29·4	0·70	49	4·53*	13·07***	48	3·85*	11·77***	0·01	30	1·06	0·01
10 and above	31·8	17·2	4·35*	138	354·67***	848·40***	45	5·48*	44·93***	47·26***	21	3·67	0·84
Total	1,000·0	1,000·0	11·47	1,000	472·76***	1,087·23***	1,000	37·92***	122·89***	75·54***	1,000	18·98*	15·96

χ^2_{AB}: Compatibility between the expected and the binomial expected (Test of homogeneity).
χ^2_A : Compatibility between the observed and the expected.
χ^2_B : Compatibility between the observed and the binomial expected.
χ^2_C : Compatibility between the observed in 5 and 10 shuffles (Test of homogeneity).

TABLE 7

Distribution of Hits (Denominations)

	Exp. No. of Trials	*Bino Exp. No. of Trials*	5 *Shuffles*			10 *Shuffles*				*Continued Shuffling*		
			Obs. No. of Trials	χ^2_A	χ^2_B	*Obs. No. of Trials*	χ^2_A	χ^2_B	χ^2_C	*Obs. No. of Trials*	χ^2_A	χ^2_B
0 and 1	40·5	27·4	109	115·86***	243·01***	50	2·23	18·64***	21·89***	29	3·27	0·09
2	84·4	70·9	86	0·03	3·22	89	0·25	4·62*	0·05	82	0·07	1·74
3	140·1	135·8	109	6·90**	5·29*	136	0·12	0·00	2·98	129	0·88	0·34
4	175·3	186·7	121	16·82***	23·12***	154	2·59	5·73*	3·96*	179	0·08	0·32
5	175·6	196·0	131	11·33***	21·56***	149	4·03*	11·27***	1·16	190	1·18	0·18
6	146·4	163·3	112	8·08**	16·12***	131	1·62	6·39*	1·49	143	0·08	2·52
7	104·5	110·9	97	0·54	1·74	108	0·12	0·08	0·59	121	2·61	0·92
8	65·2	62·4	72	0·71	1·48	84	5·42*	7·48**	0·92	66	0·01	0·21
9	36·2	29·4	43	1·28	6·29*	60	15·65***	31·85***	2·81	37	0·02	1·96
10 and above	31·8	17·2	120	244·63***	614·41***	39	1·63	27·63***	41·26***	24	1·91	2·69
Total	1,000·0	1,000·0	1,000	406·18***	936·24***	1,000	33·66***	113·69***	77·11***	1,000	10·11	10·97

(χ^2_A, χ^2_B, χ^2_C: as in Table 6.)

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TABLE 8

Pratt and Woodruff's Data: Series B

Hits	Exp. No. of Trials	Bino. Exp. No. of Trials	χ^2_{AB}	Original Data: Obs. No. of Trials	Original Data: χ^2_A	Original Data: χ^2_B	Cross-check Data: Obs. Nos. of Trials	Cross-check Data: χ^2_A	Cross-check Data: χ^2_B
0 and 1	97·20	65·76	6·07*	62	12·75***	0·21	68	8·77**	0·08
2	202·56	170·06	2·83	138	20·58***	6·04*	165	6·96**	0·15
3	336·24	325·92	0·16	301	3·69	1·91	323	0·52	0·03
4	420·72	448·08	0·86	432	0·30	0·58	454	2·63	0·08
5	421·44	470·40	2·69	471	5·83*	0·00	464	4·30*	0·09
6	351·36	391·92	2·21	374	1·46	0·82	379	2·17	0·43
7	250·80	266·16	0·46	302	10·45**	4·83*	268	1·18	0·01
8	156·48	149·66	0·15	163	0·27	1·19	158	0·01	0·46
9	86·88	70·63	1·68	92	0·30	6·47*	80	0·54	1·24
10 and above	76·32	41·41	10·35**	65	1·68	13·44***	41	16·35***	0·00
Total	2,400·00	2,400·00	27·46***	2,400	57·31***	35·49***	2,400	43·43***	2·57

(χ^2_{AB}, χ^2_A, χ^2_B: as in Table 6)

D T H H C D S T D H C T T C would give the remarkably high score of 5,530 (expected 5,000; χ^2 (1 d.f.) = 56·18***). This is not a 5 × 5 arrangement, but the following one is: S H S C D T S H C D S D T H H C D S T D H C T T C. This would still give an extraordinarily high number of hits of 5,523 (χ^2(1 df.) = 54·71***) in the very same set of 1,000 sequences. It is easily seen from Table 5 that by picking out the largest figure in each column a similar fixed sequence can be chosen for the case where all 25 cards are distinguished from each other, neither suit nor denomination being confounded. Moreover, there is a considerable number of such fixed sequences, in either case, which would give a very highly significant number of hits. Tables 6 and 7 show that, for continued shuffling, the suit hits against the initial order are 4,996, and denomination hits 5,068. Neither is significant, but this proves nothing as to the validity of *ESP* or the efficiency of shuffling. Such tests on pooled data throw away most of the information.

The 25 × 25 table enables the analysis of variance technique to be applied in great detail. This is exhibited in Table 9. The results do not differ significantly if the angular transformation be used in place of the original data, or if χ^2 be substituted for analysis of variance. The Roman numerals I, II, III and IV refer to the squares given by Fisher and Yates (1948) for the complete orthogonalization of the 5 × 5 Latin square. The two remaining sources, namely rows and columns, contribute nothing to the analysis of variance, because the total for every row and every column is fixed. In this analysis of variance, it may be noted that there are occasional stray significant effects which are not to be accounted for and are not very easy to label. But we suggest here that the precognition (Soal and Bateman, 1954) and psycho-kinesis occasionally reported by *ESP* workers may be associated with one or more degrees of freedom in such re-grouping.

Finally, there remains one important question: namely, the conclusion that may be drawn when a phenomenon of very faint probability is actually observed. For example, Soal and Bateman (1954) report observations (pooled data) whose probability according to *ESP* theorists is of the order of 10^{-70}. In Ripley (1948, pp. 142–143), a case is reported of one Mr. Leuben who shuffled a pack of cards 4,246,028 times before he made it come out in a sequence which he had fixed from the beginning as his target. The probability for

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TABLE 9

Analysis of Table 5

Source		*D.f.*	*S.S.*	*M. Sq.*	*F*
Suit × Suit	I	4	128·592	32·148	$(1·53)^{-1}$
	II	4	224·272	56·068	1·14
	III	4	217·648	54·412	1·11
	IV	4	299·728	74·932	1·52
Suit × Denomination	I	4	443·552	110·888	2·25
	II	4	718·032	179·508	3·65**
	III	4	61·808	15·452	$(3·19)^{-1}$
	IV	4	55·248	13·812	$(3·56)^{-1}$
Denomination × Suit	I	4	111·120	27·780	$(1·77)^{-1}$
	II	4	252·560	63·140	1·28
	III	4	237·440	59·360	1·21
	IV	4	784·880	196·220	3·99**
Denomination × Denomination	I	4	72·592	18·148	$(2·71)^{-1}$
	II	4	283·072	70·768	1·43
	III	4	52·848	13·212	$(3·73)^{-1}$
	IV	4	66·608	16·652	$(2·96)^{-1}$
Denomination Hits × Suit Hits	I	4	92·432	23·108	$(2·13)^{-1}$
	II	4	331·232	82·808	1·68
	III	4	207·472	51·868	1·05
	IV	4	265·248	66·312	1·35
Residual . .		496	24,421·616	49·237	—
Total . .		576	29,328·000	—	—

getting one or more sequences of fixed type in so many trials is of the order of 10^{-62}. The cartoonist drew no conclusion as to "psycho-kinesis" or the effect of the experiment as willpower upon the cards. In fact, he merely labels the experimenter as a "lunatic".

In these circumstances, it seems to us that the following conclusions are justified:

1. Card shuffling is not an efficient method of randomization, unless proved to be so by long series experiments, for the particular function of the sequence, shuffling technique, and initial order chosen.

2. Much *ESP* work (so far as may be judged from reports) is based on assumptions which are not theoretically justified.

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REVIEWS OF STATISTICAL AND ECONOMIC BOOKS

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1.—*Statistical Theory: the Relationship of Probability, Credibility and Error.* By Lancelot Hogben, F.R.S. London, George Allen & Unwin, 1957. 510 pp. $8\frac{3}{4}''$. 45*s.*

The title of this book is rather misleading. It is not an introduction to statistical theory for beginners, but rather a criticism of some standard theory and methods. In fact, it is a further consideration of some doubts and queries raised by the author in his earlier book *Choice and Chance by Cardpack and Chessboard*, Vol. 2.

Statistics is now applied in many fields—biological, physical and social sciences, commerce and industry. Nevertheless, repeated discussion at Royal Statistical Society and other meetings of the topic of statistical inference shows that statisticians are still somewhat uneasy about the basis of the methods used and the interpretation of the results, and any reasonable review and criticism in this field is welcome. Prof. Hogben has the advantage of being a trained biologist and experimenter as well as a statistician, and he is noted for his emphasis on intellectual honesty. He observes that in the past statistical theory has gone badly astray, e.g., by uncritical and inappropriate use of inverse probability. Can we assert that all is well to-day? Professor Pearson has said,

> "Probably there are several of us who can recall a considerable number of reports . . . written . . . by mathematically trained statisticians, which were hardly more than a waste of the paper on which they were written." (*J. R. Statist. Soc.*, **111**, 1948, 218.)

After such a quotation, one would expect Prof. Hogben to suggest in his book a plan for a thorough reform of either statistical theory itself or else of its teaching. Instead one finds doubts expressed about certain points in statistical theory and practice as they stand at present. But the author does not put any coherent alternative system in their place, and suggests pessimistically at the end of the book that there may be no such system.

The first three-quarters of the book are mainly concerned with the calculus of probability and its "direct" applications, which the author terms the calculus of error, of exploration and of aggregates, together with an historical review of how these came to occupy their present position. On the whole there is little here that the professional statistician could

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seriously dispute. Prof. Hogben's style of exposition is well known: the subject is illustrated by simple (but often artificial) examples, which require only simple algebra for their treatment, but the vocabulary is highly unusual, and a bit disconcerting to those used to standard terminology. In this way he hopes to bring some appreciation of the assumptions and principles underlying statistical methods to the user who may not have any advanced mathematical knowledge. The author seems to consider that statistics is generally taught at present in too authoritarian a manner, and those who need to use it ought to have a better knowledge of the background so as to apply it in a critical and understanding way. However, in 1958 very few statisticians would want to seem in any way authoritarian, and an understanding of statistical principles by the scientist is a great help to the statistician, both as regards the advice he gives, and reciprocally in enabling him to understand better just what the scientist is looking for. Any book which promotes this critical appreciation is helpful. But often the experimenter is not much interested in the statistical details (not unnaturally, for it is not his subject) and is grateful for a collection of simple recipes without bothering about finer points. Unfortunately the reviewer feels doubtful as to how well the present book will fulfil its purpose. The long discussion of elementary points will repel the professional, while the beginner will hardly know what the argument is about, and will get more enlightenment from some more suitable introductory text, such as *Choice and Chance by Cardpack and Chessboard*, if that is to his taste. This is a pity, since there are many important and shrewd comments scattered about the book, and a number of statistical sins are roundly and rightly condemned. Most of these result from insufficient co-operation and mutual understanding between experimenter and statistician.

For example, one tendency at present is to consider it the right and proper aim of an experimenter to get a result significant at the 5 per cent. level. To take a fictitious example: Dr. X constructs a 2 by 2 table, classified by rows into rheumatic patients versus controls and by columns into tomato-eaters versus tomato-haters. If he should find, say, $\chi^2 = 5{\cdot}1$, he would announce triumphantly that there is an association between tomato-eating and rheumatism, whereas if $\chi^2 = 3$, he would conclude that there is no association. Although few scientists would behave quite like that after mature reflection, it seems worth emphasizing that a 5 per cent. significance is rather a clue indicating that further experimentation is desirable, and that the investigation cannot be considered really complete until the association is not only demonstrated to exist, with practical certainty, but also the physiological mechanism underlying it has been discovered.

Another snag is that of useless calculation. Mr. Y, let us say, has laboriously gathered much data: he brings along a confusing mass of figures, and confidingly and hopefully asks what he can do with them. In the absence of better ideas there is an almost irresistible temptation to say "Well, you can do a *t*-test here" or "why not calculate the regression?", without bothering to see what interpretation these calculations might have, or whether the appropriate assumptions of normality, homoscedasticity, etc., are fulfilled. Factor analysis might well be put under this heading.

There is also the question of when data can be legitimately considered as a random sample from a population. We have information about the inhabitants of Glynceiriog: do they in any sense form a random sample of some population, and if so, which one? It is difficult to think of any convincing real or hypothetical population from which they can be said to be drawn at random. Prof. Hogben emphasizes that Nature cannot be expected to provide automatic randomizing processes. Perhaps he overstates the case. Sometimes it pays to oversimplify, and assume that a sample or process is random when it quite clearly is not. For example, "information theory" is very useful to telephone engineers, even though it depends on treating spoken language as a random process. The book will be useful reading for the professional statistician, for these and other similar comments, though one feels that even more could have been said if the author had wanted.

The last part of the book concerns "statistical inference", i.e., reasoning from observation to hypothesis. (Here, be it noted, the author is writing as a scientist, not as a manufacturer who takes a sample in order to decide whether a consignment of material should

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be accepted or rejected.) The basic principle is laid down at the beginning of the book. If I understand him rightly, Prof. Hogben argues that there is no general objective criterion by which one person's strength of belief can be shown to be better than another's. Therefore we should abandon inconclusive and interminable wrangling over such an unanswerable question, and consider people's actual behaviour instead of their beliefs. He attaches great importance to this "behaviouristic" approach.

Now in some subjects such a radical cutting out of unanswerable questions is indeed effective and useful. But here the effect is to abolish the subject itself. For the problem which a scientist will put will generally be "Here is some evidence I have got relevant to hypothesis *H*, though insufficient to prove or disprove it with practical certainty. How much faith should I have in *H*?". Prof. Hogben refuses to discuss this question in public, even though privately he may believe some hypotheses more strongly than others. This is a heroic decision to make; even if it automatically avoids the difficult disputation on questions of inference, it does so at a considerable sacrifice, and it also raises its own problems. The author would follow Neyman and Wald, and substitute "statistical prudence" for "statistical inference"; that is to say, he would use some kind of a rule which when consistently followed will guarantee that not more than a given proportion of judgments are in error. This is hardly "behaviouristic", since the judgments in question will be to accept or reject a hypothesis in circumstances in which a real investigator will (correctly) rather suspend judgment to some extent. However, while the author does not take this objection into account, he does find certain other snags in the usual theory of confidence intervals, and tries to overcome them by adding further postulates. Finally he despairs of getting any satisfactory solution, and decides on this and other grounds that there is little future in mathematical statistics. Such sweeping pessimism is honest, if unconvincing. The present astonishing growth in statistical theory would seem to indicate the contrary, even if a few of its offshoots are misguided, and others are still imperfect and not entirely logical in their application.

To summarize, one may say that his book is valuable reading for the statistician mainly in view of the awkward questions it raises about certain points in theory and application. To most of the questions it gives no clear answer: but it is as well that they should be put forward.

CEDRIC A. B. SMITH.

2.—*The Mathematical Theory of Epidemics.* By N. T. J. Bailey. London, Griffin, 1957. viii, 194 pp. $8\frac{3}{4}''$. 36*s*.

Fellows of the Society interested in the mathematical and statistical theory of epidemics will recall a valuable survey by Mr. Bailey in the 1955 volume of the *Journal* (Series B), where especial attention was paid to the statistical analysis of infection within households or other small groups. This and other recent work, including that relating to larger groups, has now been brought together in the present monograph, which is claimed to be the first systematic text-book on mathematical epidemiology.

The author's treatment, while compact, is comprehensive and up-to-date on the mathematical side. It is convenient in appraising the present position in epidemic theory to separate "small-scale" and "large-scale" epidemic studies. In the former, stochastic or probabilistic formulations of the problem are essential if unbiased and efficient estimates of unknown parameters such as infectivity or average incubation interval are to be made. These small-scale stochastic studies stem largely from the work of Greenwood and his school, independent developments in the United States by Reed and Frost being less completely stochastic in character. The detailed exposition in this book, including a numerical table to facilitate computations in the case of a particular ("continuous infection") model, is extremely valuable, though it should be added that the sets of data suitable for such precise statistical studies are at present few and far between. This is not altogether due to the early stage of development of the subject, but partly also to the difficulty of compromising between a manageable probability model and the complications of real life, a difficulty which makes the choice of a reasonable model an extremely delicate one. It is to be hoped that any misgivings on this point will not antagonize medical epidemiologists

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seeking to follow these methods, for in the context of small-scale studies at least it is a problem that has to be faced.

With regard to larger-scale studies, while these must also depend on the studies just referred to, there is more room for manoeuvre; indeed some of these studies have been purely mathematical in exploring the theoretical consequences of some particular model. Here Bailey mentions the pioneering work in stochastic models by McKendrick as early as 1926, and goes on to give an account of more recent work, in which previous deterministic models have been rediscussed in stochastic terms. It seems important to notice here that stochastic formulations do not always imply merely a slight variation on deterministic predictions, and the so-called Threshold Theorem for an epidemic in a closed population is a case in point. In the stochastic formulation, an epidemic *may*, rather than *will*, develop when the density of susceptibles is over the threshold, this leading to the U-shaped distributions for the total number of cases first investigated by McKendrick but curiously ignored in his subsequent deterministic formulation with Kermack. This divergence in behaviour can be important also in the study of recurrent epidemics and endemic infection, so that such a remark as Macdonald's in another important recent book in epidemiology: "when the groups are large the deterministic approach can reproduce happenings within acceptable limits of error" (*The Epidemiology and Control of Malaria*, Oxford, 1957, p. i of Appendix) must be regarded rather cautiously.

Bailey's book can be recommended wholeheartedly to mathematicians and mathematical statisticians interested in epidemiology, for whom it is primarily intended (for example, its bibliography only claims to be complete on the mathematical side). It is also hoped to prove of use to public health authorities and medical epidemiologists, but while, as the author tries to show, the material in his book is very relevant for the problems with which these groups of workers have to contend, it is doubtful whether more than the most mathematically minded of these will be able to tackle his book in its entirety. The solution to this dilemma, which is a familiar one in biological applications generally, is the one proposed by Bailey in his concluding sentences, of a close co-operation between mathematicians and statisticians on the one hand and clinical and epidemiological research workers on the other. This would certainly have been the solution favoured by Greenwood, who would have been delighted to see the growing interest and activity in epidemic theory exemplified by this book.

M. S. BARTLETT.

3.—*Experimental Designs*. By W. G. Cochran and G. M. Cox. New York, J. Wiley. 2nd ed., 1957. xiv, 611 pp. 9″. $10.25.

The first edition of this book, reviewed in this *Journal*, **113** (1950), 577, has deservedly established itself as a standard text-book. It is notable especially for the series of detailed plans of the more complex designs and for the clear examples of the corresponding methods of statistical analysis.

For the second edition the authors have added two new chapters and a number of additional sections and plans. Of the relatively minor additions the most important are the account of the use of Latin squares when carry-over treatment effects are expected, and the index to the numerous partially balanced incomplete block designs that have been enumerated in recent years. New topics which get very brief mention include sequential sampling and multiple comparisons.

The first of the new chapters deals with fractional replication, a device whose use has increased greatly in recent years. There is a clear account of fractional replication in the 2^n system with data from such an experiment analysed by Yates's adding and subtracting method. Detailed plans are given for 17 of the simpler designs; it is worth noting that since the book went to press the National Bureau of Standards has published an exhaustive catalogue which should be consulted by anyone wishing to embark on one of the more complicated cases. There is a brief discussion of fractional replication when all factors are at more than two levels.

The second new chapter is concerned with the application of regression methods to the study of those factorial experiments in which the factor levels correspond to values of

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quantitative variables, such as temperature, amount of fertilizer, etc. The situations when the relation between response and factor levels is first linear and then quadratic are dealt with and then methods for the estimation of optimum conditions are described.

More attention could, I think, have been paid to the difficult question of the choice of extreme factor levels in the various designs, for this has a considerable effect on the precision and bias of the estimates. Further, the choice of units in which the factor levels are expressed affects the definition of the so-called rotatable designs and also the direction of the path of steepest ascent used in estimating maxima; this last point is discussed briefly on p. 362.

The new material maintains the high standard of lucidity of the first edition.

D. R. Cox.

4.—*Techniques of Mathematical Analysis.* By C. J. Tranter. London, English Universities Press, 1957. xii, 396 pp. $8\frac{1}{2}''$. 27*s.* 6*d.*

This book is a sequel to Prof. Tranter's *Advanced Level Pure Mathematics* in the same series of books, *Physical Science Texts.* It covers series, calculus, complex numbers, De Moivre's theorem and complex trigonometric functions, algebraic equations, determinants, elementary function theory, and elementary differential equations. The author has taken great care to make the book readable, reliable and accurate. It is intended primarily for mathematics and physical science students in their last year at school and first year at university, i.e. at approximately scholarship standard, but it is well and clearly written and should be equally suited for self-tuition by any student with the necessary background knowledge.

Cedric A. B. Smith.

5.—*Probability, Statistics and Truth.* By Richard von Mises. 2nd revised English ed., prepared by Hilda Geiringer. London, Allen and Unwin. New York, Macmillan Co., 1957. xiv, 244 pp. $8\frac{1}{2}$. 28*s.*

This book contains a semi-popular account of a philosophy of probability that has been influential since 1919. The author did perhaps as much as anyone to convert many budding probabilists and statisticians to "frequentism", i.e. to the interpretation of probability in terms of long-range relative frequency.

von Mises was by no means the first frequentist; he was preceded, for example, by Venn, Cournot, Ellis, and even to some extent by the seventeenth century gambler Chevalier de Méré. The peculiar contribution of von Mises was the exposition of frequentism in terms of an abstraction called a "collective", which is another name for a random sequence.

A collective is an infinite sequence of symbols each of which belongs to a fixed finite or enumerable "alphabet" such as (A, B, . . . , Z) or (0, 1) or (0, 1, . . . , 9) or (1, 2, 3, . . . *ad inf.*), such that

(a) the relative frequency of each symbol, i, in the nth partial sequence, tends to a limit p_i when n tends to infinity ($\Sigma p_i = 1$),

(b) these limits are unaffected if the collective is replaced by an infinite subsequence by means of any "place selection" fixed in advance. (The "impossibility of a gambling system".) The number of such place selections that are permissible is best regarded as at most enumerable, since it is only in this case that the idea of a collective can be proved to be self-consistent. It is condition (b) that mainly distinguishes von Mises's philosophy from earlier naive forms of frequentism.

The author refers to several elaborate papers in which the mathematical consistency of the idea of a collective is discussed. As a matter of fact a proof of consistency can be obtained in a few sentences by reference to the Lebesgue measure of sets of "generalized decimals", as defined by the reviewer.* (It is convenient for the proof to make use of the strong law of large numbers, which can be regarded as a theorem of pure mathematics.)

* This *Journal*, Series B, 13 (1951), p. 62, and *Information Theory, Third London Symposium*, 1955 (Butterworth, *London*, 1956), p. 14.

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The mathematical existence of "combinable collectives" (i.e. independent random sequences) can be established at once by operating in multidimensional space.

In order to be able to apply the idea of a collective to real life it is necessary to be able to recognize from a finite initial segment that we are looking at part of a collective. This finite segment (partial sequence) must be long enough to be regarded as of approximately infinite length, just as, when geometry is applied, a dot must be regarded as of approximately zero size. If we ask how large is "approximately infinite", von Mises would answer (pp. 84–85) that for all scientific theories there is the problem of applicability, but that this problem inevitably cannot be formalized. For example, the notion of the density of a fluid at a point is as open to criticism as that of a collective. What this answer overlooks is that by widening the theory we may be going some way towards dealing with the problem of applicability. For probability theory this widening can be achieved by means of theories of credibility or of personal (subjective) or multipersonal theories of probability. If a philosopher with a small vocabulary were to formulate the inductive law: "Thing. Thing again", then his problem of applicability could be partially solved by enlarging his vocabulary.

The words "belief", "credibility", "utility", "moral value", and "judgment" are not in the Index. These omissions are symptomatic of the author's decision not to discuss the above problem of applicability in any detail. His criticisms of non-frequentist philosophies of probability is confined to the classical Bayes-Laplace use of equally probable cases, and there is no reference to neo/Bayes-Laplace philosophies in which the emphasis is on judgments of probability inequalities, or on choices between courses of behaviour.

Several times von Mises incorrectly supposes that the probabilities in the principle of indifference are supposed to be equal to the physical probabilities (for example, on p. 76). Moreover he does not notice that even physical probabilities can often be guessed from conditions of physical symmetry. He argues (p. 74) that the only possible way of getting any information at all about the (physical) probabilities of 1, 2, . . . , 6 for a die is by throwing that particular die repeatedly. What if the die is a dodecahedron made of unbaked dough?

The protracted arguments against formulating probability theory in terms of equally probable cases are valid only against primitive forms of that formulation. A modern formulation of this type is to use equally probable cases merely as a method of arriving at a set of axioms for probability and then to abandon any further necessary dependence on the idea of equally probable cases. To the reviewer this formulation seems to be no worse and no better than doing exactly the same by means of limiting relative frequencies. (Yet a third method is by choices between actions as, for example, in L. J. Savage's book.)

A single example will indicate the advantage of a formal axiomatic approach over the constant reference to collectives. On pp. 40–41 it is pointed out at length that the addition rule does not depend only on mutual exclusiveness, and the reader is given the impression that the difficulty can be resolved only by using the idea of a collective. It would have been simpler to say

> "Not (E and F) does not imply that $P(E \mid G) + P(F \mid H) = P(E \text{ or } F \mid G)$, if G and H are distinct propositions."

There is a firm attitude that if we talk about the probability of a single event we must mean within a collective, and that the answer will depend on the choice of collective. An example not given by the author would be to estimate a man's expectation of life by classifying him with all those men who have all his physical disabilities and possibly others. This attitude makes one wonder whether the following citation (p. 13) would be made less just by the deletion of the first occurrence of the word "not": "Thus, a probability theory which does not introduce from the very beginning a connexion between probability and relative frequency is not able to contribute anything to the study of reality".

The style is often polemical or even political. For example, on p. 158 we read "I do not understand the many beautiful words used by Fisher and his followers in support of the likelihood theory. The main argument, namely, that p is not a variable but an 'un-

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known constant', does not mean anything to me." There are also some caustic comments concerning the theory of small samples.

It is pointed out that initial probability distributions tend to become unimportant when samples are large and that uniform initial probability distributions can therefore be assumed in these cases. It would be possible to deduce from concrete examples of the use of this method some of the inequalities that would subsist between von Mises's subjective probability judgments. Such inequalities would be determined by statements of the sizes of sample required.

A very good feature of the exposition is the frequent use of summaries.

On pp. 210–211 there are some interesting remarks about causality.

On pp. 184–186 Mach is supported in his objection to Boltzmann's derivation of the Second Law of Thermodynamics. It is implied that statistical physical laws cannot be deduced from a deterministic physical theory, or at any rate not from the differential equations of classical deterministic mechanics. The argument would perhaps be more to the point in the form that we cannot get final probability distributions unless there are initial ones: we can only push probability further back and cannot get rid of it. It is like pushing consciousness and God further back. There does however remain the possibility that apparent indeterminism arises out of sheer complication, just as pseudo-random numbers may be, for most practicable purposes, indistinguishable from actually random ones. For this reason absolute determinism can never be ruled out, as von Mises and others suppose.

This is a stimulating and provocative book; especially provocative.

I. J. Good.

6.—*Statistical Tables for Biological, Agricultural and Medical Research.* By R. A. Fisher and F. Yates. 5th Ed. Edinburgh and London, Oliver & Boyd, 1957. x, 138 pp. 11″. 24*s*.

It is always difficult to write a review of tables of any kind, but this problem is intensified when something has to be said about the 5th edition of such a well-known and popular book as Fisher and Yates.

No major changes have been made either in the tabular matter or in the accounts of how to use them, and all that can be recorded are the additional tables now provided. Tables on the Logit and Complementary Loglog Transformations are now given in the same form as those for Probits. A table based on exact formulae for Behrens's test of the Difference between two means with small, odd degrees of freedom has been added. A new table is provided for the application of the product-ratio method in investigations with intercrossed data. The natural logarithm and the k-squared tables have been extended.

The various new tables were calculated on the Rothamsted 401 computer.

K. D. Tocher.

7.—*Fractional Factorial Experiment Designs for Factors at Two Levels.* By The Statistical Engineering Laboratory. Washington, Superintendent of Documents, 1957. iv, 85 pp. 10¼″. 50*c*. (Nat. Bureau of Standards Appl. Math. Series 48.)

The fractional factorial experiment designs introduced by Finney in 1945 have found increasing use in complex industrial situations where the number of factors is large and the possibilities of large experiments restricted. These experiments, of course, attempt to find out about the factorial system more than can possibly be found out from the information given by the experiment, and the objective is only achieved by making assumptions concerning the non-existence of high-order interactions. It can, therefore, be best regarded as exploratory technique rather than one from which any firm conclusions can be drawn.

The majority of the work on these designs has been for two-level designs, in which a complete theory based on Abelian groups is available. The application of this theory to design any required experiment is perfectly simple, but is very tedious, and the introduction of these tables will certainly remove a serious chore.

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The tables are described by a plan number $r \cdot n \cdot k$, which denotes a

$$\frac{1}{r^{\text{th}}}$$

replicate of 2^n factorial system in blocks of size k. r takes the value 2^p, $p = 1(1)8$. k takes the value 2^q, $q = 2(1)7$. n takes a range of values up to 16. A complete list of the plans given is provided at the front, setting out very clearly the main characteristics of the designs. The designs chosen all have the property that main effects are confounded with interactions of at least second (three factor) order, and that the number of first order interactions confounded with each other is minimized. A first order interaction aliased only with higher order interactions is termed measurable, and for each design the measurable interactions are stated. The usual notation is used and for each design the tables give the fundamental identity, the block and row confounding and the actual design (completely numerated up to $r = 32$, and described by a principal block and one element from the other blocks for r greater than this).

It is to be regretted that no account is given of the methods used for constructing these designs, in the sense that the checks performed to ensure their accuracy are not stated, but assuming their accuracy these tables make a useful contribution to the armoury of the experimental designer.

K. D. TOCHER.

8.—*Tables of the Non-Central t-Distribution.* By George J. Resnikoff and Gerald J. Lieberman. Stanford University Press, 1957. London, Oxford Univ. Press [vii], 389 pp. $9\frac{1}{4}''$. 100*s*.

This is the most complete table of this function yet compiled, and as with all multi-parameter distributions the problem arises of the most suitable parameters to use in its construction.

The authors have chosen to use the primary variable

$$x = \frac{t}{\sqrt{f}},$$

where t is the usual t-statistic and f is the number of degrees of freedom, and for the non-centrality parameter δ,

$$\frac{\delta}{f+1} = k_p.$$

The ranges of f are 2 (1) (24) (5) (49). The values of k_p used are the standardized normal random variables of exceeding the probability p, with values of $p = \cdot2500$, $\cdot1500$, $\cdot1000$, $\cdot0650$, $\cdot0400$, $\cdot0250$, $\cdot0100$, $\cdot0040$, $\cdot0025$ and $\cdot0010$. This rather curious set of non-central parameters was motivated by the principal use of the non-central t-statistic for problems involving the fraction of normally distributed random variables falling above and/or below specified values. The range of values chosen has been unfortunate in some cases, since there are several dozen pages of tables in which the entries to the whole page are all zero. All values are given to four decimal places.

The introduction to the tables gives some of the basic relationships required to extend the range of tables to parameter values not tabulated, and its application in finding the power function of sequential t-test procedures due to Wald, Arnold, Goldberg and Rushton, and also its application in sampling inspection procedures and in determining the distribution of the co-efficient of variation. These tables were computed on the I.B.M. card programmed computer, Model 2, and used the usual recurrence relation technique introduced by Rushton. This was used for values of f less than 20, but for larger values of f a steepest descent asymptotic was used. For $f = 20$ two methods were checked against each other, and the common results compared with Airey's tables.

This gave the density function which was integrated by an unstated numerical technique. Spot checks against Johnson and Welch's tables were made.

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The value of a specialized table of this kind is very difficult to judge without detailed experience of the problems that it proposes to solve. One cannot help feeling that with the increasing use of automatic computers a large number of the problems which in the "bad old days" would have been solved using such a set of tables, will now be solved *ab initio* by the computer.

K. D. TOCHER.

9.—*The Measurement of Levels of Living with Special Reference to Jamaica.* By C. A. Moser. London, H.M.S.O., 1957. (Colonial Research Studies, 24). 106 pp. 9½". 13*s.* 6*d.*

Agencies responsible for applying economic aid to less developed countries have long felt the need for some measure not only of the absolute socio-economic conditions of populations, as a guide to priority for assistance, but also of changes in such conditions as an index of the efficacy of economic aid. As the primary international agency in this field the United Nations has concerned itself very much with this problem and has organized studies and conferences culminating in a report of a Committee of Experts convened by the Secretary General (*Report on International Definition and Measurement of Standards and Levels of Living*, U.N. 1954). The U.N. report, which has received further study by international organizations, was couched in general terms: the present study forms an attempt to consider the problems in relation to a particular country—Jamaica.

It is a paradox that every social worker knows what he means by "standard of living" (and they probably all mean much the same thing) but no one can separate the whole complex of social conditions into essential, definable and measurable elements recognizable in every kind of society and comparable both from country to country and from period to period. The U.N. Report has proposed twelve components: Health, food and nutrition, education, conditions of work, employment situation, aggregate consumption and savings, transportation, housing, clothing, recreation, social security, and human freedom. Mr. Moser chose to apply himself to education, nutrition, health and housing with some less detailed reference to income, expenditure and consumption. The author's description of conceptual difficulties which he encountered in appraising the data available and of the way in which he formed a judgment of indicators will be of much value to other workers.

Mr. Moser finds it useful to classify indicators into three broad groups relating respectively to resources (schools, hospitals, etc.), utilization of resources (school enrolment, hospital attendance, etc.) and end-effects at which these resources are aimed (literacy rates, mortality rates, etc.). The case for and against a composite or summary indicator (to embody all indicators) is carefully argued. The conclusion is reached that it might be more meaningful to group the resources indices into a "social resources account" and to assess the return obtained on these resources in terms of improved social conditions. Exactly what the author proposes is not very clear and it is disappointing that this promising line of approach is not developed at any length.

In the end Mr. Moser argues, as many others have argued, that "it is misleading to compare the level of living of two countries whose climate, laws, customs, values and social and economic structure differ in important respects". His solution is to standardize each country's level of living by reference to its own target standard. The target standards are to be based on "needs" using "similar criteria". What this means is the least well explained passage in the report. But since this is, after all, the central problem it would be unfair to expect a single exploration such as this to chart completely such a wide area of uncertainty. Mr. Moser has given an excellent account of Jamaica's progress and, into the bargain, has done some sound thinking aloud. He has obviously not finished with the problem; and others who pick it up will be the better equipped for his report.

B. BENJAMIN.

10.—*Report of an Enquiry into Household Expenditure in Great Britain.* Ministry of Labour and National Service, London, H.M.S.O., 1957. iv, 304 pp. 11". 37*s.* 6*d.*

The many users of household expenditure data will all have been grateful for the decision taken in 1951 that the official inquiry which was then being planned should not be restricted

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to the provision of weights for a new price index but should be comprehensive and based on a sample of the whole population of the United Kingdom. Although the analysis now published is perhaps meagre by comparison with the eighteen volumes of data compiled by the U.S. Bureau of Labor Statistics from its 1950 and 1951 inquiries, it is by far the most comprehensive collection of household expenditure data which is available for the United Kingdom.* The main tables show expenditure on over a hundred commodities or services; and in addition to a number of one-way classifications (by household income, by income of head of household, by region, etc.) there are two-way classifications by household income and by other factors (household size, composition, occupation of head, geographical areas, type of dwelling, etc.) combined. Since there were 12,911 households whose expenditure records have been used in the analysis, most of these subdivisions contain quite a reasonable number of sample households, although it must not be overlooked in the case of some of the smaller groups that each household's expenditure record covers a period of only three weeks. Commendably the tables show for each of the subdivisions, not only the number of households on which the figures are based but also information about the average household composition in terms of sex, age groups, and employment status. There is little excuse therefore for facile comparisons between groups which take no account of the influence of such factors as these.

Another valuable feature of the report is the fact that it gives in Appendices not only the questionnaires which were used, so that one can see what information was collected, but also the item codes and sorting codes used in summarizing the records on punched cards, indicating the further analyses which could be made from those cards.

It was almost inevitable that an inquiry of this size would yield at least one item of expenditure which would prove embarrassing in terms of the statistical analysis. In this case it was a fur coat, costing £1,903, which was included in the expenditure record of one of the 78 sample households with an income of £50 per week or more. This has led to a proliferation of footnotes against the item for women's outer clothing, to explain the very odd figures each time this household occurs. It would perhaps have been more helpful if these footnotes could have included a statement of the mean expenditure in each of these groups omitting that particular household (it can, of course, be calculated if one assumes that this was the only purchase of women's outer clothing by the household during the three-week period, but this is somewhat tiresome). An alternative approach which might have been used is to regard fur coats, jewellery, yachts, perhaps even motorcars, and a range of other articles above a certain value (separately fixed for each) as coming within a separate commodity group of "luxuries". One would expect some odd results for such a luxury group; even so it would seem closer to reality to estimate that households with more than £50 a week spend about £8 a week or more on such a composite group than that they spend nearly £10 a week on women's outer clothing.

Since most of the analysis is by income group, in combination with other factors or alone, it may be as well to see what that implies. The answers to the income questions seem to have been one of the less satisfactory parts of the inquiry, as is so often the case in studies of this kind. Indeed, viewing the results of the inquiry as a whole, its most obvious biases arose from the characteristic understatement of income and of expenditure on alcoholic drink and tobacco. In the published tabulations all these are shown as stated by the respondents, and the division into income groups has been made without any adjustment for the understatement of income. Relatively few of the households giving expenditure details seem to have refused income information (it was incomplete for over 500 of the 12,911 whose expenditure records were used) but the report says that there must have been many cases of understatement. This is based both on the fact that expenditure (including some forms of saving) appreciably exceeded income, which *could* be partially due to overstatement of the former, and also on comparisons with information from other sources. It adduces the usual reasons for understatement:

* A detailed analysis, mainly by household size and by total expenditure, of part of the Ministry of Labour's 1937–38 material and of the Civil Service Statistical and Research Bureau's 1938–39 data is given in Prais and Houthakker, *The Analysis of Family Budgets*, Cambridge: The University Press, 1955.

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(i) A tendency to give net, rather than gross, figures,

(ii) omission or understatement of overtime pay, bonuses, subsidiary earnings, property incomes and casual receipts,

(iii) the fact that people working on own account often give figures for an earlier period.

Somewhat surprisingly, in view of these causes, it is also said that "there is no reason to suspect that . . . understatement was much more prevalent or relatively greater in one income class than another". Unfortunately, the report does not attempt to analyse incomes by their sources, so does not itself provide information about the distribution of the incomes which are most liable to understatement; but one would have thought from other experience that understatement of property and own-account incomes in the upper income ranges would have been relatively more important than the other omissions and understatements—especially since property incomes constitute a group where there is often marked confusion between the gross and net concepts. However this is of relatively little importance in relation to this particular report since the income figures are used only to place households in income groups, and this probably does amount to a rough grouping in order of magnitude provided one is not too concerned about the exact order or the exact definition of the boundaries of the groups.

This comment on income understatement is nevertheless one of several statements on "side" issues which seem to be inadequately supported by the material presented. It is said (para. 123), in relation to the analysis by income group and by occupation of head of household, for instance, that "it seems probable that there are certain ingrained differences in the spending habits of different social classes that are independent of income level". This is of course a supposition which is frequently made and which it would be interesting to test, but it certainly cannot be deduced from the context in which it occurs. This compares the distribution of expenditures of the households of professional, clerical, etc. workers and of manual workers each in the £10–14 a week income group; but the differences in average household composition between these two groups are so great that it would be very rash to infer anything at all about the influence of the occupational factor, even if the income distributions within the groups were similar—which seems improbable.

Of a slightly different kind, is the statement (para. 130) that to compare the "cost of living" in two different regions "it would be necessary . . . to establish and compare the total cost of this fixed 'basket' of goods and services in the various regions". This is a possible interpretation of "cost of living" but it is one which completely evades all the well-known difficulties of comparing standards under different conditions (e.g. in this case, of journey to work, quantity of fuel and warm clothing required, and so on). And it is quite unnecessary to the argument of the paragraph! Somewhat similar is the inaccurate explanation (para. 20) of the reasons for adopting the standard Social Survey technique for dealing with multi-household dwellings in a rating list sample. Here a reference to the well-known paper by Gray and Corlett "Sampling for the Social Survey" (this *Journal*, 1950) would surely have been more satisfactory, and appropriate.

There are other small points in the presentation which are rather puzzling. Possibly the oddest is that no statement of the mean expenditure of all 12,911 households taken together is given until Table 45 on page 224. Since this table is really concerned with seasonal variation (it groups households according to the period in which they kept records), it seems likely that the more general user may miss it altogether and may be led to construct his own table of the overall means by combining the figures from one of the earlier analyses. Another unusual point, which one would have thought worthy of textual comment, is the use of *gross* Schedule A valuations for the rental value of owner-occupied dwellings, although actual expenditure on repairs is also included in housing expenditure. No doubt this is justified by the generally low level of Schedule A valuations; it seems a pity not to have said so. No such simple explanation can be suggested for the treatment of income from sub-letting. This is deducted from the rent, rates and water charges paid by the main tenant, who is thus not credited with any income from this activity unless the rent he receives exceeds his total outgoings on these items. Where owner-occupiers sub-let,

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the rent was similarly deducted from the total of rates and water charges, ground rent, insurance on the dwelling and the gross Schedule A valuation; but no deduction was made from the owner's expenditure on repairs, even where the rent received exceeded the sum of the foregoing items. Admittedly any treatment of rent from sub-letting must be arbitrary, but it seems to be carrying arbitrariness to the extreme of contrariness to adopt a convention which implies that most people who sub-let do not do so for gain.

These are all minor matters which do not affect the usefulness of the considerable body of data which has been assembled. That is indisputable; but every individual will have his own ideas about other items which might have been included but are not. To state a personal preference, it would have been valuable to have analyses by the age of the head of household in combination with other factors and an analysis of recorded expenditure for the first, second and third weeks' records separately. It was unfortunate that the information collected did not provide for the statement of age if it was between 21 and 60, which would have been valuable for a study of the life-cycle in income and expenditure; but even within this limitation some analysis of the households with people over 60 would have shed new light on the economic problems of ageing. The analysis of "pensioner" households excluded from the retail price index is made more valuable by distinguishing one-person and two-person households, but these are by definition all *poor* old people and this tells us nothing about the relative position of different groups of the elderly. The comparison of the separate weeks' records is, of course, of methodological interest in relation to the optimum period of record-keeping in such an inquiry. Perhaps we may hope to see some findings on this subject in some other form.

There is one further omission from the report which is hardly a question of personal preference and which almost all serious users will deplore. Nowhere is any indication given of the variability of the various items within the groups which have been chosen for presentation. It is said (para. 92) that the publication of sampling errors "would give a spurious appearance of precision to the results and accordingly no figures of this kind have been included in this report", but a statement of the means without errors is surely equally likely to be taken to imply precision. In any case, sampling errors convey much more than an "appearance of precision"; and, if it was felt that the use of the term "error" would be misleading, there are other measures (variance or even inter-quartile range) which indicate the dispersion without having the same connotation for the inexpert. This is not a case of wanting to know the reliability of the sample means as an estimate of the "true" means, but of having some indication of the individual variations which one is ignoring when one uses a mean of any kind whatever.

Finally, a point about authorship. This is one of a number of publications (*National Income Statistics: Sources and Methods* was another) which might well have been made an exception to the general rule of anonymity in official publications. They do not contain any statements of policy to which the doctrine of Ministerial responsibility must apply; yet, by their very nature, they are almost bound to contain some expressions of opinion (such as those quoted above) which are in fact personal rather than official and which are the more acceptable if this is made clear. A volume like this is obviously the work of many minds and it may be that civil service practice is such that it would be impossible to name any small number of persons as the authors in the usual sense. But, even if this is so, there would seem to be a case for mentioning those who have been chiefly responsible for assembling and publishing so valuable a collection of data. The present volume does not even name the members of the Technical Committee which advised on the methods of the inquiry.

J. E. G. UTTING.

11.—*The Registrar-General's Statistical Review of England and Wales for the Year* 1955, *Part III Commentary*. London, H.M.S.O., 1957. xiii, 274 pp. 9¾". 11*s*.

Regular readers of the Registrar-General's Reviews will remember that, before the Second World War, circumstances conspired to prevent the non-medical (or civil) part of the text being either attractive or particularly enlightening. The situation changed so

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little from year to year that there was no opportunity to release a new and startling fact, the statistics available were relatively trivial and, despite the growing awareness of the danger of the steadily declining birth rate, the interest of the public was far less in fertility and nuptiality than in their own chances of achieving a ripe old age. The collection of additional statistics from July 1st, 1938 under the Population (Statistics) Act of that year so enlarged the possibilities of analysis that annual texts from that date might well have developed into a new and more attractive form, but the intervention of the war prevented this, and for a time completely changed the situation.

To catch up with arrears occasioned by staff shortages and the wartime "statistical blackout", and for other reasons, the civil commentary from 1938 to 1950 was compressed into 3 volumes—for 1938–39, 1940–45 and 1946–50—and these being periods packed with demographic change, there was no lack of action on which to report. The text for 1951 was thus the first embracing the civil events of only a single year since the additional information collected under the 1938 Act became available and the civil commentaries from 1951–55 may be examined for such indications as they give of the Registrar-General's future intentions. One tendency, which may be expected to be of a permanent nature, is for greater condensation and compression and this will probably, in general, be welcomed by most users. For instance, the tedious distributions of maternities by duration have been dropped, no doubt on the grounds that the irregularities visible are likely to be more indicative of chance variations in the exposure-to-risk than due to changes in the pattern of family building. There are, however, dangers of over-condensation and, though these are no doubt well known, they cannot be over-emphasized. One danger is that it may lead to a commonly used index failing to appear in its accustomed place, or even failing to appear at all. These indices will frequently be extracted from each volume by a user studying the secular trend, and the discontinuation or even break of a series is a great disservice. An example is the marriage rate for women aged 15–49 which, having been shown in serial tables, may be extracted for the period 1851 to 1954 by consulting only four text volumes. This index for 1955 does not appear at all in the 1955 volume. (Is the objection to it that it is not age-standardized? Neither is the crude birth or death rate.)

Another trend which may be seen is the introduction of a procedure regularly employed by the medical statisticians. Having so many different causes of death on which to report, the medical commentators have been able to select a few topics for special study each year, and to change these so that a topic may not reappear for several years. By this time some event of interest has had time to appear, so that the commentary is again interesting and avoids the boring practice of treading continually the same route with a barely perceptible change in destination in each successive year. The topics of the civil text lack the natural profuse subdivisions of the medical text, so that the same procedure cannot so easily be followed. Nevertheless there are indications that it is intended to attempt the application of this principle to the civil text and this will no doubt be approved by the majority of users. Many topics worthy of occasional study cannot usefully be analysed annually (sex ratio at birth, twins, seasonal trends, regional differentials, etc.) and the inclusion of a detailed examination of these features on a rota would add considerably to the interest of the civil texts. In connection with the possibility of periodic inclusion of detailed commentary on some topics, it may be stressed that the outsider needs an index to guide him to where he may find commentary on particular topics.

Finally, the re-introduction of generation and marriage cohort analyses of fertility into the 1955 commentary will be noted with satisfaction, particularly as it would appear that some progressive summary figures are to become a regular feature of the reviews.

If these trends are indicative of changes that may be expected in the next few years as a more definite form for an annual text is evolved, there is little doubt that future civil texts will in general be more readable and useful to the demographer. But what of the workers in fields abutting demography, of which there are many? With the current spate of scientific papers, few such workers have time to read more than the summary of reports not precisely referring to their own field, a fact recognized by the Registrar-General who now produces such a summary. These summaries could, however, be criticized as showing signs of having been produced in haste by lifting sentences from the full text when, out

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of their context, they may fail to give a true picture. The reference, in the 1955 summary, to more divorced men remarrying than divorced women may be instanced. This appears to have been lifted from p. 60, where it introduces statistics showing as their salient feature, *not* this numerical superiority of males, but that the current trend is towards a *reduction* of this superiority. Again, the summary devotes but three short paragraphs to all aspects of fertility, and gives no indication that this volume contains—in the text and appendices—outstanding contributions to statistics on cohort and generation fertility. Since this summary will be the sole source of demographic information to so many, considerable care would seem to be called for to see that it is the salient and significant features which find their way into it.

The demographic content of the 1955 volume of particular interest is as follows:—

Population

Some tables, which had customarily appeared in the population section, but had disappeared in recent years, have made a welcome return, e.g. the national population by sex, age and marital condition. An interesting analysis is included of the sex-age structure of Regions, etc., in broad but meaningful age groups. It is hoped that, as an occasional study, the Registrar-General will enlarge on this.

Fertility

Much of the earlier type of treatment has been progressively reduced in recent volumes and, as mentioned above, further reductions are shown by the present text and may be applauded. New analyses showing cohort and generation fertility have been appearing, and this trend is extended still further in the present volume, no doubt partly stimulated by the 1951 Census Fertility volume which is apparently about to be published. These types of analysis are now so firmly established that it can be presumed without question that the overwhelming majority of serious demographers would approve of the changes being made. A tentative suggestion is made that current fertility may be running somewhat above replacement level in striking contrast to the suggestion of several writers recently that there was a deficiency of some 5 per cent. Without the details of the calculation, which are to appear in the 1951 Census Fertility volume, further comment on the point cannot be made. This new section on cohort fertility of the 1955 Commentary should be studied by all serious demographers.

It should be noted that bringing together results from the 1946 Family Census, vital statistics and the Census of 1951, the Registrar-General has produced, and publishes in an Appendix, invaluable basic cohort statistics, showing average family sizes of marriage cohorts by single calendar years (back to 1929) for single years of duration (to 30) and quinary ages at marriage.

A new and useful analysis is supplied by a comparison of the fertility of first marriages and remarriages. It would appear that this is a tentative first attempt and further development along these lines may be expected. If the data will stand it, it would be interesting to compare the fertility of remarriages according to the number of children born in previous marriages.

Of the minor fertility sections, seasonal variation is given more prominence than is usual, and this is perhaps an analysis that will appear only periodically.

Marriages

A useful attempt is made to clarify the remark that has been made for many years that "high marriage rates are depleting the population of young spinsters", by showing an analysis of the annual change in the spinster population by age.

1955 was a year of exceptionally high marriage rates even in the context of current high rates, and the usual diagram shows this, though the aggregated rate has disappeared.

The section, "Factors Influencing Marriage", includes an improvement. Referring to the changing sex ratio of the young population at risk of marriage, recent texts have shown

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data only for census years, the most recent being 1931 and 1951. Thus there was no indication of current trend i.e. of the tangent to the curve. The 1955 volume shows the 1955 position, deduced from estimates, and thus meets this need.

The proportion of women in each age group of status "married" continues to rise, though marriage rates have fallen below their highest post-war point. The significance of this situation has been pointed out repeatedly by the Registrar-General, who indeed virtually predicted the fall in marriage rates. A question the reader would like answered is, what is the relationship of current proportions married to the proportions in a current nuptiality table? It would appear that the Registrar-General does not calculate nuptiality tables each year, since he discusses this very point in somewhat guarded phrases. It could be that there is sufficient demand to warrant his calculating and publishing such tables.

Many diagrams that have appeared in this and earlier volumes have been excellent, but a three-dimensional perspective of the trend in age structure of married women (p. 52) is not a success. The third-dimension, calendar time, is of little value, and its inclusion reduces the ability to read accurately the changes in structure.

The sections on seasonal incidence and local variation in marriage are much reduced, and no doubt will receive particular attention from time to time in future volumes.

Divorce

The divorce section is short and follows the pattern of recent years. A fuller section appeared when the Royal Commission on Marriage and Divorce was set up, and no doubt the civil statisticians have been too preoccupied since to make another detailed study. The Registrar-General will also, no doubt, be aware of analyses under way by other bodies and would not wish to duplicate their work. However, the time must be nearly ripe for a review of the existing pattern of analysis. Cohort analysis would be enlightening, particularly since so unexpectedly the incidence of divorce is falling each year—only falling trivially, but past experience would suggest a rise after the temporary effect of the Legal Aid and Advice Act had passed. The sections on remarriage of divorced persons (appearing in the marriage section and in the divorce section) are unsatisfactory. In the marriage section, rates should be shown separately for the widowed and divorced—despite the difficulties in estimating the appropriate population at risk. In the divorce section, remarriages each year are related to the divorces of the same year—a valid analysis to give a rough and ready guide to the proportion of divorced persons who remarry, but the ratios so obtained are shown in the text compared from one year to the next. In view of the recent rise in the general level of divorce, the total numbers of the population who are divorcees is presumably rising, and in consequence so is the number remarrying. This virtually ensures that in general the above mentioned ratio will rise, which is what it is doing, but without any implication whatever that the proportion of divorced persons remarrying is actually rising. No doubt the same clarity that has been entering the fertility analyses of the text will in due course enter the divorce section also.

General Mortality

The Registrar-General wastes no space speculating on the vexed, but purely academic, question as to when the trend of the crude death rate reached its lowest point before commencing its inevitable rise—a question which can be answered for certain only in the future—but the usual excellent serial tables showing standardized, unstandardized and age-specific death rates readily enable the reader to make up his own mind. The period 1950–55 seems likely to include the answer, i.e. the crude death rate may now well be rising on average. A more important point is that even standardized death rates seem to be stationary, the continuing decline at ages under 65 being offset by increases at older ages.

Cause Mortality

A feature chosen for special attention is an extremely brief but enlightening broad review of the *relative* importance of various causes of death at different ages. A diagram (p. 69) shows at a glance the considerable importance of accidental death for males up to

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middle age and for girls to adolescence, of diseases of the nervous and circulatory systems from middle age, and of cancer throughout life. The brevity of this analysis enhances rather than detracts from its value.

Infant Mortality

The usual full study is modified this year to show particularly the relative stagnation in perinatal mortality—the 1955 rate (37·4) is by chance identical with that for 1950. in contrast with declines of post-neonatal mortality (1950 rate 11·1, 1955 rate 7·6) though even here stagnation may be approaching (the 1954 rate was 7.7) because the rate has been reduced to such a low level.

A special study of maternal mortality is also included, despite its virtual eradication in the last 20 years (1933, rate 5·89 per thousand births, 1955, 0·81, p. 100) but the refusal of medical statisticians of complaisance and sheltering behind arguments of "hard core" mortality is well known. For the layman, however, a conscious effort is necessary if a sense of proportion is to be retained, hence the value of the analysis of p. 69, referred to above.

Tuberculosis

The usual full analysis shows clearly the continuing decline in this disease under the pressure of measures taken against it. Such factors as changing diagnostic efficiency make the study of this disease one in which the layman would be well advised to study carefully the Registrar-General's commentary, rather than to examine only the numerical data.

Cancer

Public awareness of the rising trend in cancer of the lung is of such recent origin that the layman might expect the Registrar-General's most recent commentary to contain an *unusually* detailed study of the subject. The facts are, however, that the Registrar-General has for many years drawn attention to this situation, and a number of special studies will be found in the reviews for past years. The particular analyses chosen for the present volume include excellent graphs (pp. 139–141, 143 etc.) but the layman should study the text also for an appreciation of such questions as diagnostic efficiency.

The Registrar-General is depressing, though no doubt realistic, when he hazards (p. 142) the suggestion that (whereas death rates from other diseases will decline until presumably the "hard core" is reached) death rates from cancer will increase until "saturation" is reached. He does not say so, but it is clear that he is discussing the lessons to be learnt from current trends. Clearly a discovery, analogous to that of antibiotics, would introduce a discontinuity, and the trend thereafter would be unrelated to the current trend.

Accidents

A serial table for 1931 to 1955 (p. 164) shows the continuing rise in death rates of young men from motor accidents; for ages from 15 to 24 the rates in 1955 are the highest shown in the table. Other later tables show tabulations of deaths by type of accident and cause of death, and show 81 per cent. of motor cyclists killed die from head injuries other than with motor vehicles, a higher proportion than occurs to any other road user. Other analyses examine the secular trend of this proportion with particular reference to any improvements deriving from the growing popularity of crash helmets (p. 169).

Miscellaneous

Besides the usual miscellaneous tables, there are tabulations of multiple causes of death with particular reference to tuberculosis, diabetes and cancer. These were prepared in collaboration with World Health Organization as a guide to National Committees on Vital and Health Statistics, and have been included as "of sufficient national interest for inclusion". These analyses show details of the deaths with mention of, but not assigned to, each of the above causes.

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Nineteen per cent. of deaths with mention of tuberculosis, 66 per cent. of deaths with mention of diabetes and 6 per cent. of those with mention of cancer are not assigned to these causes.

N. H. CARRIER.

12.—*Tuberculosis Statistics for England and Wales*, 1938–1955: *An Analysis of Trends and Geographical Distribution*. By W. P. D. Logan, and B. Benjamin. G.R.O. Studies on Medical and Population subjects No. 10. London: H.M.S.O., 1957. iii, 56 pp. $9\frac{3}{4}''$. 4*s*.

Although tuberculosis is no longer "captain of the men of death", it would be foolhardy to assume that it can now be disregarded or ignored. Any signs of complacency towards the disease are immediately dispelled by the facts presented in this informative and timely report.

Until recently, epidemiological studies of tuberculosis have been based largely upon mortality rather than morbidity statistics principally because of the greater degree of completeness of the former. However, with the rapid decline in the number of tuberculosis deaths in this country during the past few years, mortality rates no longer give as reliable a picture of the situation as hitherto. On the other hand, statutory notifications are known to be deficient because of the undetected and unknown cases in the community. Despite such deficiencies (which, it should be noted, have been reduced in recent years through the introduction of the mass radiography service) it is now necessary to use notifications as a measure of prevalence since no other suitable yardstick is available.

In males, the respiratory tuberculosis notification rate reaches a peak at ages 15–24. Thereafter it tends to fall with increasing age, but there is a secondary peak in middle life. For females, the peak occurs in the same age groups as for men, but in contrast to the male experience, the rate in females after age 25 declines rapidly. The non-respiratory notifications are largely incomplete, but, for what they are worth, the incidence is highest at the youngest ages and declines to a very low level beyond middle adult life. The highest notification rates during 1954–55 occurred in the Northern, Midland and London and South-Eastern regions and Wales, while the lowest was the Eastern region. In each regional area, the sex-age distribution of notifications was broadly similar to that for the country as a whole. There was a large excess over the national average for males at ages 15–24 and 65 and over in the London and South-Eastern region and in Wales—the two regions with the highest rates for males of all ages.

Death rates generally rise with increasing age in both sexes and in non-respiratory as well as respiratory forms of the disease, but the gradient is not as steep for non-respiratory forms of the disease as for respiratory. Up to 25 years of age the respiratory mortality follows a parallel trend for both sexes, with female mortality showing a slight excess. Over 25 years, the male mortality continues to increase with increasing age, in contrast to the female mortality which remains fairly constant.

The work of mass radiography together with the improved chest facilities, and a more co-operative public attitude have combined to make the notification rates following the last war not comparable with earlier years. Although it is difficult to distinguish the real pattern of events, nonetheless, there have been certain interesting sex and age differentials both in morbidity and mortality which are brought out clearly in the diagrams and tables of the report.

Reference is briefly made to overcrowding, occupational mortality, the extent of invalidity and the administrative measures for tackling the problem. It is staggering to learn that the total loss to national productivity in Great Britain each year in respect of those covered by sickness insurance alone is 26 million person-days. Moreover, tuberculosis still affects the younger ages, and though most recover there may be, as the authors point out, "impairment of function and lack of ability to return to occupations as vigorous as those adopted prior to illness".

Some idea of the current magnitude of the problem can be obtained from the following facts. At the end of 1955 there were 307,182 notified cases of respiratory tuberculosis

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in England and Wales of whom 19,323 were known to have tubercle bacilli in the sputum in the preceding six months. The pool appears to be increasing at the rate of about 11,000 per year; the average annual number of notifications over the period 1951–55 was 39,000 and the average number of deaths, 8,400. Apart from the known infector pool, there is probably (based on mass radiography experience) some 75,000 unsuspected cases. This pool is being fed by new lesions (probably 50,000 cases a year) and reduced by transfers, those who recover without being discovered and deaths. The net annual reduction is only of the order of a few thousand.

In the face of these facts, it would be quite erroneous to conclude that tuberculosis is no longer of national or social importance. The authors have rendered a service to all those particularly interested in the problem by presenting the salient features in such a clear, concise and simple form.

A. BARR.

13.—*Chronic Bronchitis in Newcastle-upon-Tyne.* By A. G. Ogilvie and D. J. Newell. Edinburgh and London, Livingstone, 1957. vii, 115 pp. 8½″. 15*s.*

Chronic bronchitis as an incapacitating disease especially in middle and later life is one of the outstanding public health problems of the day; the cost to industry in sickness absence is staggering—for males in Great Britain it is of the order of 20 million days a year, and is the cause of one-tenth of all sickness absence covered by national insurance—and the distress to the individual patient is no less a serious medical problem. As with earlier problems much patient statistical documentation is required before the community consciousness is so impressed as to require the marshalling of adequate medical resources and other social action to deal with this problem. The present survey by a medical specialist and a statistician adds much substance to this documentation.

The authors first assemble such facts as are available from national statistics to illustrate some international differentials and also differentials within England and Wales which point to the need for a study of aetiological factors—the starting point of their survey.

Aetiological studies cannot be carried out without prior diagnostic definition and two chapters are appropriately devoted to the morbid anatomy of the disease and to difficulties of standardizing diagnostic description, and of how these difficulties were resolved in a satisfactory working convention.

The rest of the book is largely statistical, describing the methods of survey (interviews by health visitors and subsequent medical examination of a sample population) and the results of the inquiries. The sample population was divided into bronchitics and a non-bronchitic control population of similar age and sex constitution, and the role of various factors was assessed by comparison of these two segments. Observer error in history recording was not left out of account.

Smoking (but not the number of cigarettes smoked), working conditions involving extremes of temperature, draughts and dusty atmosphere (but no particular occupation), and prolonged residence in sooty, damp and foggy surroundings (but not housing itself) were factors associated with bronchitis. The high incidence of bronchitis in Social Class V was thought to be economic rather than occupational.

The authors conclude that infection is the dominating factor in chronic bronchitis but this is maintained and aggravated by other environmental agents. Elimination of these agents might well lead to arrest or healing of the process during the early (reversible) stage before the stage of progressive (and irreversible) obliterative bronchiolitis is reached. At present one-third of cases pass to this second stage. These facts are challenging.

B. BENJAMIN.

14.—*Morbidity and Mortality in the First Year of Life.* Edited by Fred Grundy and E. Lewis-Faning. London, The Eugenics Society, 1957. 145 pp. 8½″.

The survey of infant sickness and mortality in fifteeen areas of England and Wales reported in this book is an attempt to answer some of the questions raised by the work of Professor Grundy and Dr. Dykes in Luton after the war, in particular the suggestion of an

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important constitutional element in infant sickness. The fifteen areas include thirteen towns and cities and two rural areas.

The data about infant sickness referred to the 21,000 children born in the fifteen areas in 1952; the infant deaths were the 2,000 which occurred in the same areas in the three years 1951–53. Information about sickness was collected by health visitors from the mothers, and that about mortality was compiled by medical officers of health.

The morbidity enquiry revealed a social gradient in sickness with high rates amongst the children of unskilled and semi-skilled workers, falling to the lowest rates amongst the children of professional men, the range being some 50 per cent. of the mean. The gradient was almost wholly attributable to the respiratory and acute infectious diseases, which, indeed, accounted for well over half of the new spells of sickness. Various factors which are strongly associated with social class were then considered and the inception rate of new illnesses was shown to be related to all of them; the rate was high where standards of housing, maternal care and parental education were low, amongst premature babies, and in the largest families. These factors did not, however, explain all the gradient. Similar findings apply to mortality—though relationships were generally stronger and gradients steeper. Very low mortality rates were shown for favoured groups, and very high ones for "illegitimate" children. (The word "illegitimate" is used in a rather special sense, births to unmarried couples living together as man and wife and who married before the end of the survey being classed as legitimate.) It was judged that some 6 per cent. of all deaths could have been avoided by better or prompter medical care.

An interesting chapter on congenital malformations is contributed by Dr. C. O. Carter who points out that only 62 per cent. of all malformations recorded in this group were noticed at birth, the remainder being diagnosed during the first year of life. This brings out the importance of at least a year's observation for study of malformations. Twenty-six per cent. of the infant deaths in this enquiry occurred in malformed children and the evidence suggests that deaths attributed to congenital malformation on death certificates underestimate the true contribution to mortality in the first year by about one-fifth. Still-births were not included in the enquiry and anencephaly was therefore hardly represented. No class or seasonal factors could be shown to be associated with the presence of malformation, and the only association with maternal age was the well-known one with mongolism. Case fatality for all causes was very high in malformed chldren, and was highest in the poorer classes.

Separate chapters are also devoted to immaturity and to acute respiratory disease. The high mortality in immature children persisted to the sixth month of life, and the social gradient in deaths due to immaturity was almost entirely because more immature babies were born in the poorer classes rather than because those who were so born were more likely to die. Mortality rates from acute respiratory disease were higher in the early months than later, but the excess mortality in early months did not explain the class gradient in mortality from these infections since the proportion of early deaths was very similar in all classes.

The previous work on sickness in infancy from an epidemiological point of view is sufficiently scarce for the publication of a book on the subject to be something of a landmark—and to invite comparison with one of the few others, *A Thousand Families in Newcastle upon Tyne* (Spence *et al.*, 1954). The two books could hardly be more different. In Newcastle the stress was on accuracy of diagnosis and intimate knowledge of the family. The result was exceedingly good clinical information on a group of children, which, though large by clinical standards, was not large enough for much cross-tabulation of relevant factors. The present study handles twenty times as many children—with the result that some at least of the second order relationships can be demonstrated—but one cannot help but feel that it is at the cost of the quality of the clinical material. Ascertainment in this study, and perhaps to a lesser degree diagnosis, depend essentially on the mother's statements as interpreted by the health visitor. The authors are extremely frank about the limitations of the data, and perhaps it is not quite cricket to "use their statements in evidence". Nevertheless it must be said that a "memory factor" is demonstrated but its importance in terms of bias is not explored. It is clearly shown that the evidence is not

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equally reliable from all areas. The information on maternal age (and surely this must be amongst the easiest data to obtain) is discarded as unsatisfactory without explanation. The classification of mothers according to the quality of the care they gave, even though reliance is only placed on extremes, must be subjective and possibly biased in favour of middle-class standards—moreover some of it must have been after the event. The findings on this particular factor are of course in general agreement with Spence (whose assessment is also open to criticism on grounds of objectivity), and indeed with most other workers in this field. Another assessment, that of avoidability, was almost bound to be conservative in the circumstances.

In spite of these remarks this book is essential reading for all those interested in the social aetiology of infant sickness; such readers will be indebted to the authors and their many collaborators for the lucid presentation of a great deal of very interesting material. It is particularly interesting, in the light of the work in Luton, that a social gradient in sickness was clearly shown, and that the steeper gradient in mortality was shown to be due partly to this class gradient in illness and partly to a class gradient in case fatality.

J. A. HEADY.

Reference.

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15.—*The Aging of Populations and its Economic and Social Implications*. United Nations, Department of Economic and Social Affairs. New York, 1956. vii, 168 pp. 11″. 12*s*. 6*d*. (Population Studies, No. 26.)

Like its predecessors, this latest study by the demographic staff of the United Nations is clear and comprehensive. It examines a problem in greater detail and over a wider field than could fairly be expected from the work of any individual expert or national organization, and by reason of this is able to develop research further than before along already-established lines of enquiry. As the title indicates, the present volume is concerned with age-distributions and their variations, and the effects they have upon such factors as production and consumption. It does not attempt to deal with special issues such as biological aging or the employment of elderly persons—these are regarded as being within the province of other international organizations such as W.H.O. and the I.L.O.

The study of aging in the mass suffers in two ways from the lack of a really suitable phraseology. First, the word itself has a well-understood meaning in relation to an individual person but one that is misleading in relation to a whole population; an increase in age-distribution is a more complex matter than an increase in age. Secondly, there is no satisfactory expression for the process of an age-distribution becoming younger ("rejuvenation" is used in the book but it has for some people too much of an association with one particular aspect of human activity). Demographers are often accused of inventing jargon for their own purposes, but here is a case where an addition to the language is genuinely needed.

As they have not attempted to coin any new phrases, the United Nations demographic staff have devoted some space to an explanation of the difference between personal aging and group aging. In doing so they have distinguished quite simply between an increase in age-distribution attributable to a fall in the proportion of young people and one associated with a rise in the proportion of old people; in other words, aging "at the base" or "at the apex" of the population pyramid, as they call it. In fact, however, such a pyramid is nothing more than a histogram for each sex turned over on to its side, and there is a very well-known treatment for expressing the characteristics of such a histogram in terms of its mean, its variance and the functions of its higher-order moments. In view of the strange shapes that wars can carve along the sides of pyramids, a more comprehensive statistical treatment than the authors have provided would perhaps have been justified. A similar comment might be made concerning the simple broad-banding of population into the three age-groups 0–14, 15–64, and 65 and over, that occupies much of the book, although

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so far as measuring the effects on economic functions are concerned this treatment can be defended on the score of lack of precise knowledge of the relations between such functions and age.

Factual statements and tabulations relating to present and past populations in every part of the world occupy about half the manual, and all who are concerned with demographic data will use this conveniently-presented information with gratitude.

A question that has been much discussed recently is that of the relative contributions of declining mortality and declining fertility towards population aging. It has been established that the comparison for this purpose of stationary populations is of little value, because by reason of their stationary nature they differ from one another not only in longevity but also, to an equal and opposite extent, in fertility. "Stable" populations, in which constant fertility of any required size may be combined with constant mortality of any required size, are free from this objection. Alternatively, if these are regarded as being too artificial, recourse may be had to population projections. Both methods are thoroughly investigated in the present book, and combinations of many alternative assumptions covering the whole range of recorded human experience are examined. From the results it can clearly be seen that a decline in fertility is almost always associated with an increasing age-distribution. Changes in migratory movements and in mortality have relatively small effects, but appreciable aging does result from declining mortality in the special circumstances of a low fertility and an already high longevity—those found in Great Britain and other Western Countries to-day.

The economic and social implications of aging are discussed with particular reference to participation in the labour force and to consumption and distribution. These are the subjects that lend themselves to this kind of econometric analysis—they are those in respect of which there is some detailed knowledge of how the factor is associated with age. Suitable data on other important matters, savings for instance, have yet to be collected. Participation in the labour force is discussed in terms of the proportions recorded as economically active. A paragraph or two on sickness absence from work might usefully have been added here—statistics relating to this are available at least for Great Britain. Consumption is analysed according to type of need—for instance food, housing, schools and hospitals. In the section on distribution there is an interesting analysis of the effect of aging on various social systems for the support of the old, ranging from the family group, as in primitive man, to the complex social security arrangements of the developed countries. P. R. Cox.

16.—*The Demography of Ceylon.* By N. K. Sarkar. Colombo, Government Publications Bureau, 1957. [v], 289 pp. 9¾″. Rs. 10.45.

Demographically, and in other ways, Ceylon is a prototype of Southern Asia and, more than this, is an outstanding example of a country in modern times embarking on the demographic revolution from high to low birth and death rates—the revolution followed in the Western World in the past. Thus a study of Ceylon is of relevance outside her borders.

In the first two chapters of his book, Sarkar describes the historical background of the ancient culture of Ceylon—based on a vast irrigation scheme, now mostly in decay—and estimates the order of magnitude of the population in early times from the numbers that the food supply could support. It is unlikely that his methods could be applied directly elsewhere, but it is useful as an example of what can be done with unpromising material.

Moving on to more recent times, for which a spate of estimates, censuses etc. may be found, he develops techniques applicable to this situation to sift the good from the bad figures. These techniques are certainly, to a greater or lesser extent, applicable to the population statistics of many countries, excluding only those of the statistically most sophisticated and the most primitive. Recognizing this general applicability of his results the author devotes the whole of Chapter 3 (46 pages) to a discussion of methodology. His methods are, of course, based on the familiar iterative adjustment of the recorded figures to improve internal consistency.

It may easily be argued that the techniques Sarkar uses are arbitrary and statistically crude but this is not altogether a fair judgement. In the first place, it can hardly be

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argued that he fails to reduce the error in the data, and when for administrative purposes figures must be provided, surely figures with lesser error are better than those with greater error. Secondly, apart from limited and *ad hoc* work, Sarkar's investigation which he now records, but carried out some years ago, was one of the first systematic studies of methodology in the field of rectifying population data of statistically under-developed countries.

The next four chapters—on Marriage, Fertility, Mortality and Public Health, are a study of Ceylon's demography mainly from census and vital statistics. The author employs, of course, his adjusted figures, and the analyses are more sophisticated and detailed than one normally finds for a statistically under-developed country. The chapter on Mortality examines the spectacular decline in Ceylon a decade ago and shows that the much quoted attribution of this to a decline in malaria alone is far from correct. As the author recognizes, inability to examine cause mortality by age is a serious handicap.

Another interesting feature of the mortality chapter is the production and application of a method of selecting periods in England's history in which her mortality was "equivalent" to that of Ceylon in selected periods. The technique suggests that as recently as in 1920–22, the death rate of Ceylon was as high as that of England a century ago, but in the next 25 years Ceylon brought about the mortality decline that took England 50 years. Then came the steep decline in Ceylon's mortality, making, in two years, the progress that took England 25 years. In general qualitative terms, most authors forecast a different pattern of demographic revolution in the future from that of the past: this analysis gives a quantitative measure of the difference, of possibly wide relevance at least in Southern Asia.

Later chapters on the ethnic structure of Ceylon's population tell the story of the plantation labour from India, leading on naturally to "The Economic Background" describing the growth of plantation economy, absorbing the best land and driving the rice-growing peasant economy on to the poorer land and preventing its expansion to support the increasing peasant population. The theme of increasing pressure of population in the peasant sector is shown to involve subdivision of holdings, consequent inefficient cultivation, resort to the money-lender and finally dispossession.

Looking to the future, Sarkar considers the implications of the enhanced rate of population growth from recent mortality declines and foresees disaster unless the complementary fertility decline is brought on without delay; he regrets the colonization schemes (resettling surplus population by repairing the ancient irrigation works) as dangerous palliatives which, by reducing Malthusian checks, will encourage a rise in the birth rate and absorb the increased production by increased population.

The solution Sarkar puts forward is industrialization, the process which encouraged the demographic revolution in the western world. He considers this in all its aspects—power and transport requirements, economic prerequisites and social implications. Whether he is right or wrong, his arguments are well reasoned and honest and, far from overlooking the interaction of economic and demographic factors and recognizing the dynamic nature of population growth, a discussion of these features is a major theme of Sarkar's study.

N. H. CARRIER.

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STATISTICAL NOTES

(1) British Official Statistics

The index of retail prices compiled by the Ministry of Labour and National Service which was 108 in November remained at the same level in December, January and February, though there was a fractional fall in February, due mainly to a decline in some food prices. The detailed figures of the weights used in calculating the index and the indices for different commodity groups was as follows:

	Food	*Alcoholic Drinks*	*Tobacco*	*Housing*	*Fuel and Light*	*Durable Household Goods*	*Clothing and Footwear*	*Transport and Vehicles*	*Miscellaneous*	*Services*	*All Items*
Weights:	350	71	80	87	55	66	106	68	59	58	1,000
Nov. 12th, 1957	104·8	105·7	107·8	115·9	115·3	101·3	103·0	112·7	111·1	114·0	107·7
Dec. 10th, 1957	106·0	105·7	107·8	116·9	115·6	101·3	103·0	112·7	111·6	114·2	108·2
Jan. 14th, 1958	105·4	105·8	107·8	117·7	115·6	101·4	103·0	112·8	112·4	113·1	108·1
Feb. 18th, 1958	103·9	105·8	107·8	118·1	115·5	101·4	103·3	112·8	112·4	113·2	107·6

The Ministry of Labour index of weekly wage rates, calculated on the basis of January 31st, 1956 = 100, showed that in November 1957, the level was 111·9. It rose to 112·1 in December, 112·2 in January and 112·5 in February. In manufacturing industries alone the figures were 112·0, 112·1, 112·2 and 112·5. The principal classes of workpeople whose rates of wages were increased in the period included building and civil engineering employees, operatives in ready-made tailoring, boot and shoe manufacture, hosiery, textile bleaching, dyeing and finishing and carpet workers, and agricultural employees in Scotland.

The six-monthly inquiry into average earnings showed that in October 1957, the average earnings per week in the industries covered by the enquiries, i.e. the manufacturing industries and some of the principal non-manufacturing industries and services, were 251*s.* 7*d.* for men, 129*s.* 6*d.* for women, 108*s.* 4*d.* for youths and boys and 85*s.* 2*d.* for girls. These figures represent increases since April 1947, of 104, 92, 129 and 112 per cent. For all workers combined the average was 212*s.* 5*d.*, an increase of 105 per cent. This compares with an estimated rise in weekly wage full time *rates* of wages in the same industries and services of about 75 per cent. In manufacturing industries alone the average weekly earnings in October 1957, were 261*s.* 2*d.* for men and 129*s.* 11*d.* for women. The corresponding hourly earnings were 65·3*d.* and 37·7*d.* The average hours worked per week in all the industries and services were 46·4, slightly less than at any date since April, 1954.

The total working population and the numbers in civil employment in the four months ended January 1958, were as follows:

(*Thousands*)

	Total Working Population			*Numbers in Civil Employment*		
	Males	*Females*	*Total*	*Males*	*Females*	*Total*
Oct., 1957 . .	16,254	8,027	24,281	15,400	7,925	23,325
Nov., 1957 . .	16,238	8,026	24,264	15,369	7,918	23,287
Dec., 1957 . .	16,218	7,930	24,148	15,319	7,812	23,131
Jan., 1958 . .	16,213	7,914	24,127	15,306	7,790	23,096

The number of persons on the unemployment registers of the Employment Exchanges rose by 19,000 in December, by 60,000 in January and by 29,000 in February. The total for January represented 1·9 per cent. of the total number of employees in Great Britain.

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The percentages in the different Regions ranged from 1·3 in London and the South-East and the Midlands to 3·6 in Scotland and 3·8 in Wales.

The following is the sex analysis of the figures:

Number of Unemployed Persons on the Registers of Employment Exchanges

	Men and Boys	*Women and Girls*	*Total*
Nov. 11th, 1957 . . .	218,968	97,555	316,523
Dec. 9th, 1957 . . .	235,227	100,242	335,469
Jan. 13th, 1958 . . .	282,388	113,139	395,527
Feb. 17th, 1958 . . .	301,532	123,015	424,547

Of the total of 424,547 in February, 91,358 had been unemployed for less than two weeks, 129,544 for two to eight weeks and 171,918 for over eight weeks, while 31,727 were temporarily stopped. In the five weeks ended February 12th, 1958, 184,474 vacancies were filled by the Employment Exchanges. The number of vacancies unfilled at this date was 208,942.

The number of insured persons absent from work owing to sickness, including self-employed as well as employed, were 959,100 in November, 936,400 in December, 1,076,900 in January and 1,053,500 in February. The numbers of employed persons absent owing to industrial injuries were 52,000, 48,200, 52,200 and 57,000.

The *Ministry of Labour Gazette* for February 1958, contains estimates of the total number of employees (employed and unemployed) at May 1957, based on the insurance cards exchanged in the subsequent three months supplemented by returns from employers. The totals show, for the United Kingdom, 22,326,000 (14,499,000 males and 7,827,000 females) and for Great Britain 21,850,000 (14,200,000 males and 7,650,000 females). A full analysis of the figures by industries is given.

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(2) Other Statistics

Statistics of National Income and Expenditure (Statistical Papers, Series H, No. 10. New York, U.N., 1957. 22*s.* 6*d.*) continues the efforts of the United Nations Statistical Office to publish national accounting figures in a standard form for as many separate countries as possible. The standard form adopted is, of course, that of the *System of National Accounts and Supporting Tables*, and the Statistical Office's task is made easier by the increasing extent to which various countries have adopted this as a framework for their own national accounts. The present volume sets out to present nine of the standard tables of that system, for each of the years 1950–55, with the help of the national statistical offices concerned.

The figures for each country are presented in a separate country chapter, and are preceded by some general notes as to source and the general extent to which the estimates conform to the definitions of the standard system. More detailed points of definition appear in footnotes to the tables. Altogether there are 64 separate country chapters, although the complete set of nine tables is given for only 14 of them, and certain aggregate estimates (gross national product, domestic product, net material product, etc.) are given for a further 17 countries.

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UNESCO has recently published another volume in its series devoted to the university teaching of the social sciences, *The University Teaching of Social Sciences: Statistics.* UNESCO, Paris, 1957, 209 pp., 14*s*. This extensive survey was prepared and edited on behalf of the International Statistical Institute by Professor P. C. Mahalanobis with the help of the Indian Statistical Institute and reports covering another twenty-four countries.

The report is divided into two parts: a general survey of thirty pages followed by a survey of the individual countries. The general survey reviews the objectives in the teaching of statistics—as part of general education in addition to more specialized study—together with the availability of education and training in the subject as well as the organization of teaching, curricula and research. A final section deals with problems that are raised by the general survey and suggests possible priorities for their treatment. The survey of individual countries contains a vast amount of hitherto uncoordinated information and makes fascinating reading for anyone interested in the details of comparative statistical education and training.

Bulletin 19 *of the Statistical Society of New South Wales* marks the tenth Anniversary of this lively body. A note by Helen Newton Turner, the first President of the Society, draws attention to the progress made in bringing together those, of all disciplines, interested in the statistical method and in fostering theoretical development, research, and education. It is an impressive record.

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STATISTICAL AND ECONOMIC ARTICLES IN RECENT PERIODICALS

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Vol. 23 (1958), No. 1—General resolution of correlation matrices into components and its utilization in multiple and partial regression; *J. A. Creager.* Error of measurement and the sensitivity of a test of significance; *J. P. Sutcliffe.* Determination of parameters of a functional relation by factor analysis; *L. R. Tucker.* The inclusion of response times within a

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stochastic description of the learning behavior of individual subjects; *R. J. Audley.* Determining the degree of inconsistency in a set of paired comparisons; *H. B. Gerard* and *H. N. Shapiro.* Properties of the item score matrix; *A. G. MacLean.* Predetermination of test weights; *P. J. Hoffman.*

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Vol. 5 (1957), No. 1—An ornithological application of line-grid sampling; *M. Masuyama.* On the optimality of balanced incomplete block designs; *M. Masuyama.* Notes on statistical applications of the information theory. III; *M. Sakaguchi.* On the optimality of latin-, Youden- and Shrikhande square designs; *M. Masuyama* and *T. Okuno.* Bias included in the estimator of standard deviation using range; *S. Shimada.* On difference sets for constructing orthogonal arrays of index two and of strength two; *M. Masuyama.*

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Since the issue of Part I, 1958, the Society has received the publications enumerated below.

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SERIES A (GENERAL)

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Industrial Production and Steel Consumption

By Sir Robert Shone, C.B.E., and H. R. Fisher

[Read before the Royal Statistical Society, March 19th, 1958, the President, Sir Harry Campion, C.B., C.B.E., in the Chair]

I. Introduction

The 1954–62 *Plan*

1. The British steel development plan is related to a view of the growth of the British economy over the period 1954 to 1962 (Iron and Steel Board Special Report, 1957). It incorporates as a key feature an assessment of the possible growth of industrial production over the period, and of the steel required to support this growth.

2. The plan was taking shape in 1955 and starts from the position of the steel industry and the national economy in 1954. With the first three years of the period completed and the half-way stage approaching, it is possible to make some appraisal of the reasonableness of the forward demand estimates and of the view taken of the general growth of the economy on which they were based. It also seems useful to explain in outline the technique used in arriving at this view and interpreting it in terms of steel requirements.

3. Fig. 1 shows how the plan is at present working out.* Line A shows the estimated growth in home steel consumption over the eight years. Line B is higher by about a million tons. It was considered desirable to have capacity to meet a possible demand at the level of B to provide for the normal annual stock increase associated with expanding consumption, and for the inevitable fluctuations in demand for steel which occur both because consumption varies and because consumers and merchants can substantially exceed their normal demand for steel for stock. The margin included for possible fluctuations above the expected trend was about 4 per cent. The graph also records the actual home consumption of steel up to 1957, the total deliveries of steel both for home consumption and for stockbuilding, and the deliveries for export. Curve C shows the expected level of export demand.

4. From the middle of 1955 to the middle of 1956 the level of steel consumption rose substantially above the expected trend. This was due to the coincidence of a heavy demand for durable consumer goods, particularly motor-cars for the home market, with an upsurge in industrial building and increased investment in plant and machinery. By the middle of

* Notes on the graphs and tables are given in Appendix B.

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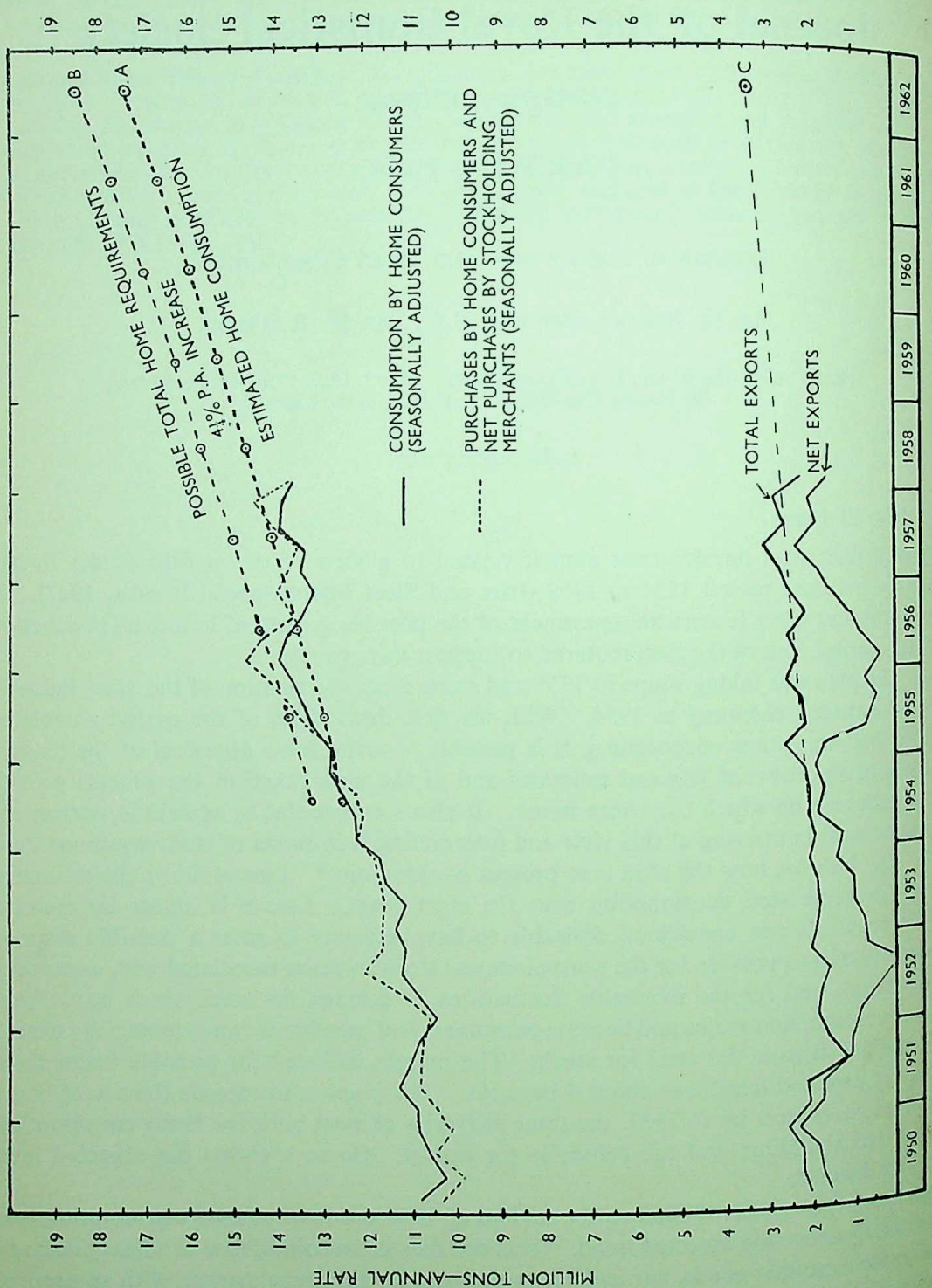

FIG. 1.—Steel supplies for home use and export (Million tons—Annual rate) (Finished weight).

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1956 hire purchase restrictions and other action had sharply reduced the domestic demand for cars and the total consumption of steel was falling. By the autumn of 1957 the effect of these restrictions had largely worked itself out, or in some cases the restrictions had been modified, and consumption of steel was rising again towards the trend line.

5. The further tightening of credit and the rise in the Bank rate in the autumn of 1957, as well as the deflationary forces at work in the world generally, seem likely to renew, at least for a time, the tendency for steel consumption to fall below the trend line.

Growth of the Economy

6. Table 1 shows the rates of growth of the main elements in the national economy assumed in making the forward estimates. Both gross national product and industrial production were expected to show a rather lower rate of growth in the eight years from 1954 to 1962 than had been experienced in the previous six years from 1948 to 1954. This was because a lower rate of growth of industrial working population was expected.

7. The results in the first three years of the new period have fallen below the estimates. To take one of the key figures as an example, the increase of industrial production per head of the industrial working population, that is to say of productivity, was estimated at 3 per cent. annually and so far has turned out to be at an average rate of only 2 per cent.

8. It is true that for the past two years there have been restrictions in force of one sort or another. Credit has been progressively tightened, interest rates have been rising, and for a considerable part of the time hire-purchase restrictions have been more severe than they had been for some years previously. And there has been concern about the balance of payments. So that the questions arise whether the economy has been over-extended, even at the lower rates of growth recently achieved—or whether the lower rates of growth have themselves been partially responsible for the over-extension—and whether doubt is now thrown on the estimates of growth used in the steel plan.

TABLE 1

Increases in Population, Industrial Production and Steel Consumption in the United Kingdom and Countries of the European Coal and Steel Community

	Average Annual Percentage Rates Compound					
	United Kingdom			*E.C.S.C.*		
	1948–54 *Actual*	1954–57 *Actual*	1954–62 *Estimated*	1948–54 *Actual*	1954–57 *Actual*	1955–65 *Estimated*
1. Increase in gross national product (for U.K., G.D.P.)	3·3	2·4	2·8	6·9	6·3*	4·1
2. Increase in working population	0·5	0·4	0·5	3·4	2·5*	0·6
3. Increase in gross national product per head of working population	2·8	2·0	2·3	2·6	3·8*	3·5†
4. Increase in industrial production	4·4	2·6	3¾‡	10·0	8·8	4·9
5. Increase in industrial working population	1·3	0·6	¾‡	6·0	2·7	0·9‡
6. Increase in industrial production per head of working population	3·1	2·0	3·0	4·0	6·1	4·0
7. Increase in steel consumption	4·0	3·5	4·2	9·7	9·6	4·7

* 1954–56.
† Expected to fall to 2·6 per cent. in the period 1965–75 because of reduction in hours worked.
‡ Estimate based on the expected transfer of labour to industry.

FIG. 1.—Steel supplies for home use and export (Million tons—Annual rate) (Finished weight).

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E.C.S.C. Estimates

9. Beside the rates of growth for the United Kingdom Table 1 shows the corresponding rates in the countries of the European Coal and Steel Community over the six years to 1954 and those assumed for the decade to 1965 (E.C.S.C. High Authority, 1957). The rapid increase in gross product and industrial production in the post-war period on the Continent was partly due to the exceptional increase in working population. This was made possible by a reduction of unemployment and by immigration. In addition productivity, particularly in industry, rose more rapidly on the Continent than in the United Kingdom. Steel consumption increased very nearly in proportion to industrial production.

10. For the decade to 1965 the estimates of growth exceed the British estimates. An increase of industrial production of 4·9 per cent. is expected compared with the British 3·8 per cent. This is not based on any further exceptional growth of working population, but on a higher rate of productivity growth than in the United Kingdom. The Community also allow, in addition to the figures shown in Table 1, a margin for upward fluctuation in steel demand of 10 per cent. in 1965.

Elasticity of Steel Consumption with Respect to Industrial Production

11. A striking feature of the United Kingdom estimates which emerged when the details were worked out was the relation indicated between steel consumption and industrial production. Over the six years 1948 to 1954 the elasticity of steel consumption with respect to the index of industrial production (that is the ratio of their proportionate growth rates) had averaged 0·9. In the forward plan it has been estimated that steel consumption will grow at a rate implying an average elasticity of over 1·1 with respect to industrial production. So far the actual ratio of growth rates, as shown in Table 1, has been fully up to this. Over the three years it has been of the order of 1·3. To a considerable extent this more rapid growth in steel consumption has offset the disappointing growth of industrial production itself and meant that steel consumption at the end of 1957 was running fairly close to the estimated trend (Fig. 1).

12. In contrast, on the Continent it has been assumed that steel consumption will grow slightly less rapidly than industrial production. Thus to accept a rate of elasticity greater than unity for forward planning in the United Kingdom is at variance with experience in the period up to 1954 in both areas, and with the assumption made in Western Europe about the future. This question of the relationship between industrial production and steel consumption will be examined more fully in the main body of the paper. In particular, the examination will show how far it is necessary in dealing with steel demand to look behind the one value-weighted total of industrial production.

13. Towards the end of the paper we shall return to the basic problem of the rate of growth of industrial production in Britain.

II. A General Relation to Industrial Production

14. Attempts have sometimes been made to relate steel consumption or supplies over the past with some general index of national economic activity, and in particular with a broadly based index of industrial production.

15. Fig. 2 shows steel supplies in the United Kingdom plotted against industrial production from 1903 to 1957. Similar data for the United States are presented in Fig. 5

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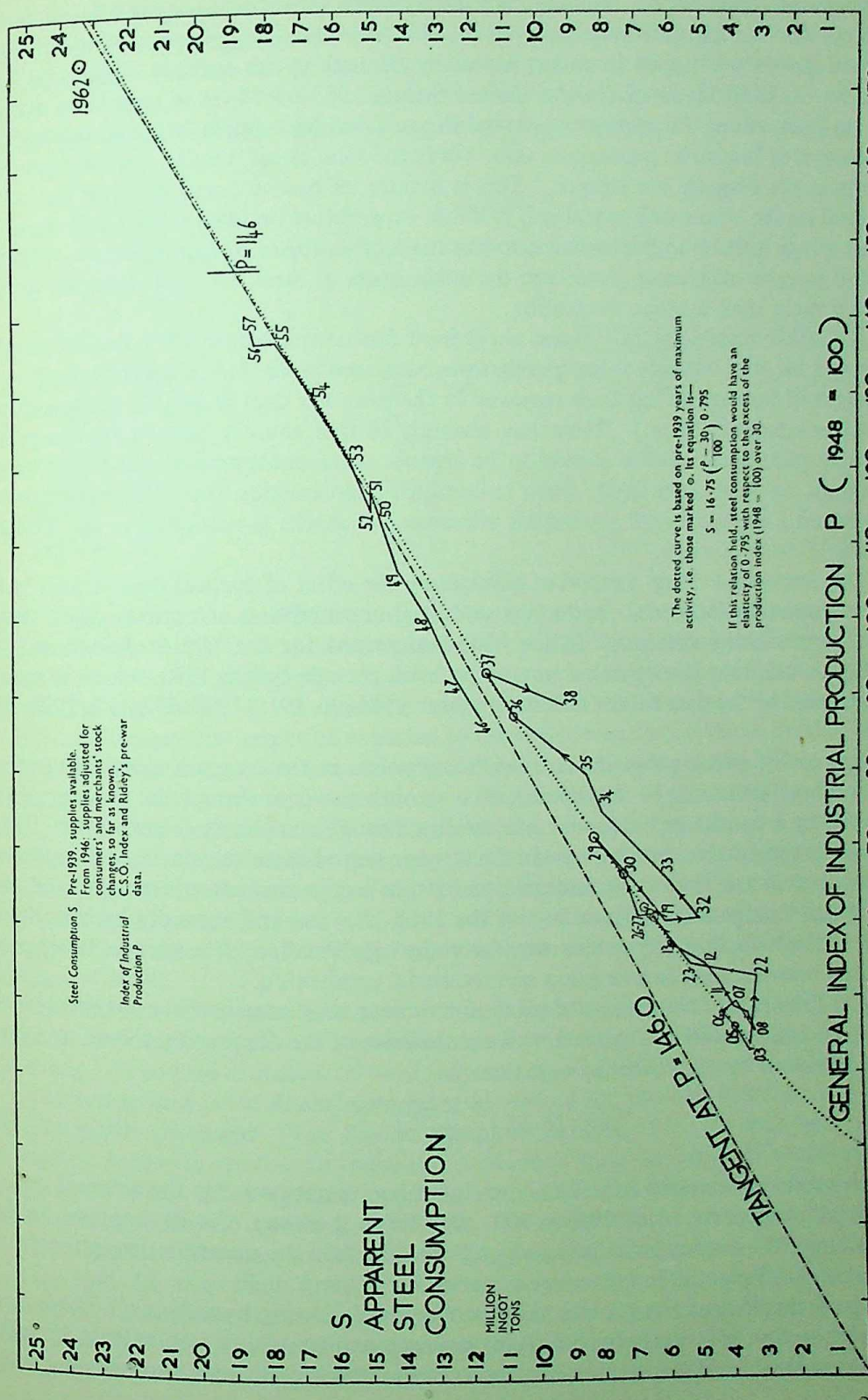

FIG. 2.—Steel consumption and total industrial production, United Kingdom 1903–1957.

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and discussed in Appendix A. Among the features which the records for the two countries show in common is the difference between the short-term and long-term movements. If the plotted points are traced in order, especially through cycles such as 1906–08–13 or 1929–32–36 or 1937–38–39 or (in the United States) 1953–54–55, it is seen that during short-term fluctuations the movement of steel demand has been much larger in relation to the movement of industrial production than it is in the long term; that is, the lines joining the points in the diagram are steeper. This is mainly, of course, a reflection of the well-known (and on the whole well-explained) fact that expenditure on fixed capital and durable consumer goods is liable to fluctuate more than most other forms of expenditure. Capital goods and durable consumer goods are the main users of steel, so steel demand shows similarly a more than average fluctuation.

16. A further reason for this excess short-term fluctuation before 1939 was no doubt that demand for steel—that is to say purchases—fluctuated more than actual consumption. (This source of fluctuation has been removed in the post-war data in Fig. 2, consumption not purchases being plotted.) There has certainly in this country been a tendency for additions to purchasers' stocks of steel to be greatest when consumption has been exceptionally high, at least since 1948, when collection of information on consumers' stocks was placed on a regular basis. A similar movement of stocks is suspected in the United States and Western Europe.

17. One somewhat crude method of eliminating the effect of cyclical fluctuations is to confine attention to industrial production and steel consumption at periods when these are at or approaching maxima. In the historical record for the United Kingdom, set out in Fig. 2, one may distinguish a number of such periods before 1940, which it seems fair to represent by the data for the following years: 1905–06, 1913, 1920, 1924–25, 1928–29, 1936–37.

18. It is rather striking that the corresponding points in the diagram, notwithstanding the intervening fluctuations,lie very closely on a smooth curve, as though the two variables were linked by a consistent functional relationship, though not simply a linear one. The consistency is remarkable, first because in the 30-year period there were many adventitious influences such as the rise of the internal combustion engine and electric power, and the industrial and foreign-trade dislocations of the 1914–18 war—and secondly because it is, after all, difficult to find any convincing theoretical justification of a unique functional relationship between steel consumption and industrial production.

19. It is found that for the years of maximum or near-maximum activity just mentioned (the corresponding points are marked with small circles in the diagram) the data are very nearly reproduced by the following equation:

$$S = 16{\cdot}75 \left(\frac{P - 30}{100}\right)^{0{\cdot}795} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where S is steel consumption in million tons ingot equivalent and P is the general index of industrial production, with 1948 = 100. As Table 2 shows, the fit happens to be extremely close, the discrepancies being a good deal less than the uncertainties in the data, which we can hardly put at less than one per cent. for either S or P.

20. The form of equation (1) was suggested by the following hypotheses. There is a considerable section of industry (including for example, textiles) which uses, at least directly, practically no steel, and this section has shown very little growth over the period. It is

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represented in the equation by the 30 subtracted from P. The remaining and growing section of industry represented by $P-30$ tended as it grew to use less steel per unit of output—either because of structural changes in the sense that industries that are light users of steel grew faster than industries that are heavy users per value unit, or because within particular industries steel was used more economically.

TABLE 2

Steel Consumption Calculated from Industrial Production in Years of High Activity

Years	*Industrial Production* 1948 = 100 P	*Apparent Steel Consumption (Supplies Available)* m. Ingot Tons S	*Calculated Steel Consumption* $16{\cdot}753\left(\frac{P-30}{100}\right)^{0{\cdot}795}$
1905–06	47·2	4·135	4·137
1913	57·3	6·00	5·972
1920	62·2	6·81	6·809
1924–25	61·0	6·57	6·607
1928–29	68·85	7·91	7·904
1936–37	90·15	11·19	11·168

21. That an equation with three adjustable parameters—the constant factor, the constant deducted from P and the exponent—should give a close fit to six selected pairs of observations is not perhaps surprising. It certainly cannot be taken as proving that the notions suggesting the choice of the equation are correct, or that the equation expresses some stable mechanism that might be expected to continue in operation into a further period.

22. It is therefore somewhat remarkable that this equation, derived from the pre-war data, gives a very close approximation, as Fig. 2 shows, to the observations for 1951, 1954, 1955, three recent years in which there was full employment and a practically free market in steel. This fact does give the equation some empirical standing as a description of a trend observed over a long period. Some corroboration—though still empirical—may be found in the American data, the short analysis of which given in Appendix A discloses a long-term relationship of a similar type, with an exponent practically the same—0·785 instead of 0·795.

23. The pre-1940 departures in the United Kingdom seem to be mainly of the kind already described as typical of general economic fluctuations; and no more need be said of them. War-time estimates of total industrial production have not been obtained and would in any case be of doubtful meaning in view of the profound changes in the nature of the goods produced. The United Kingdom emerged from the war with steel consumption higher in relation to industrial production than the previous trend would have suggested. The reason appears to be the simple one that engineering and shipbuilding had been expanded to meet war-time needs, and other industries contracted.

24. The steel shortage in the United Kingdom lasting till about 1949, and recurring about 1951–52, may have contributed to the post-war tendency, shown by the 1946–51 record in the diagram, for steel consumption to increase at a distinctly lower proportionate rate than industrial production, and so for the balance between the two to come nearer to what could have been expected from extrapolating the long-term pre-war trend.

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25. The equation implies an elasticity of steel consumption with respect to total industrial production of

$$0{\cdot}795\,\frac{P}{P-30},$$

which, starting from $+\infty$ in the earliest days of steel usage, approaches 0·795 as P increases indefinitely. At a level of $P = 146{\cdot}3$, which is about the rate of industrial production likely to be reached during the next few years, the elasticity would be 1.

26. However in 1955–56 there was a sharp movement away from the trend line. Notwithstanding a slight reduction in total industrial production, the use of steel continued to increase. The departure from the trend was approximately maintained in 1957. This departure, following a year of indubitable maximum industrial activity, 1955, is quite contrary to pre-war habit.

27. The last point on the diagram, in the extreme top right-hand corner, shows the figures adopted for 1962 for the purpose of the Iron and Steel Board's Development Plan. This too, so far as the relation between steel consumption and industrial production is concerned, lies well above the trend, whether that trend be derived from pre-war data in the form of equation (1), or inferred from post-war data up to 1954. The 1962 estimate implies an increase in the ratio of steel consumption to industrial output, although as the graph shows the two had moved practically in proportion from 1950 to 1954.

28. To explain what has been happening since the war, and what is expected to happen, the different forms of steel-using activity must be considered in more detail. There is no magical formula that will allow us to work blind.

III. The Economic Pattern of 1962

Estimation of Steel Use by Sectors

29. To detect the changes in economic structure and techniques that affect steel demand, it is necessary to classify the national economy into sectors. The choice of classification is largely dictated by the statistics available. Apart from this, and the obvious desirability that sectors likely to show differing rates of change should be treated separately, the sectors forming the basis of the analysis should be those whose futures can be rationally estimated. This last criterion, though it must always apply, has not the same effect in all circumstances. In the study of steel demand in the United Kingdom two slightly different analyses have been developed, and are maintained side by side, to meet the needs of long-term estimation and short-term estimation. The latter will not be discussed here.

30. In the long term, that is to say if one is looking more than about five years ahead, there is no rational procedure but to construct a self-consistent model of the national economy as it might be in a future year, or better, a number of alternative models, each with an implied time-series of self-consistent models leading up to it. Whatever of hope, prudence or hunches may be incorporated in any adopted model, the range of possibilities is narrowed by the considerations that the national accounting identities must be respected, as well as any known established organic relationships. Exports must be adequate to balance imports and provide a current foreign balance sufficient to meet capital commitments. Domestic investment must be assumed to have been at a level which will permit the implied growth of production. Labour productivity must not show an implausible long-term increase. The way in which consumers are assumed to dispose of additional income must reasonably accord with experience of their choice in the past. Yet in all

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these matters the possibility of changes which are more favourable or less favourable to a dynamic economy must not be overlooked.

31. Arguments and calculations on these subjects are necessarily conducted in terms of the entities made familiar in this country by successive National Income Blue books and Economic Surveys.

The Economic Model for 1962

32. In considering the situation for which it would be prudent to ensure a future steel supply, the principle adopted was to look at the possible development of the economy with all reasonable optimism. It seemed right to assume a continuing high level of employment (which implies an increase of working population about ½–1 per cent. a year) and technological advances proceeding as rapidly as in the past. At the same time, the rate of advance chosen must not be so high that it can be seen to be rendered unlikely by other limiting factors, such as those which did in fact halt the rapid expansion of 1955. As the key figure the index of industrial production was taken; after the implications of various rates of increase in this index had been assessed, an average increase at the rate of 3¾ per cent. annually was adopted. How this looks in relation to past increases of production and increases in working population may be seen from Table 3.

TABLE 3

Annual Percentage Rates of Increase in Industrial Production at Various Periods*

	Increase—Per Cent. Compound a Year		
	Industrial Production (1)	*Industrial Working Population* (2)	*Difference* (1)–(2) (3)
1924–29	3·2	0·4	2·8
1929–37	3·4	0·9	2·5
1946–48	7·2	11·6	−4·6
1948–51	5·4	1·9	3·5
1951–54	3·4	0·8	2·6
1954–57	2·6	0·6	2·0
1954–62 assumed . .	3¾	½–1	About 3

* Mining, manufacturing, building, gas, water, electricity. The pre-1948 data are partial only.

33. In assessing what is reasonable, it is relevant that the flow of technological improvements available to industry shows no sign of stopping. And there is clearly a pool of improvements waiting to be drawn on. The figures chosen, and in particular the 3 per cent. per annum increase expected in industrial productivity, seem to strike a fair balance between hope and realism.

34. In Tables 4A and 4B are shown the interrelation of the different elements of national income and outlay, and especially the imports, exports and investment which emerged from the basis chosen. Table 4A balances home supplies and their disposal. Table 4B is the account of current foreign transactions. When amalgamated, with imports and exports each cancelling out, the two make a national income and expenditure account. They constitute a framework within which speculation on the future must be confined—although they are not, of course, the only such restraint.

35. Only the most summary account can be given here of the process by which the

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TABLE 4A

National Supplies and Expenditure

Values are in 1954 *Factor Cost*

Supplies from Home Production and Imports

	1954 £m.	1962 Total £m.	1962 Increase on 1954 £m.	1962 Increase on 1954 %
1. Work and services in "index of production" industries	7,710	10,351	2,641	34·25
2. Other work and services	7,961	9,165	1,204	15·12
3. Total above = Gross domestic product	15,671	19,516	3,845	24·54
4. Imports—				
(*a*) Merchandise	3,006			
(*b*) Other goods and services	590			
(*c*) Total	3,596	4,692	1,096	30·48
5. Total supplies	19,267	24,208	4,941	25·64

Disposal of Supplies

		1954 £m.	1962 Total £m.	1962 Increase on 1954 £m.	1962 Increase on 1954 %
Consumers' Expenditure—					
6. Durable goods		743	1,186	443	59·6
7. Vehicles		200	396	196	98·0
8. Other		9,330	11,150	1,820	19·5
9. Total		10,273	12,732	2,459	23·9
10. *Public Authorities' Current Expenditure on Goods and Services*		3,035	3,317	282	9·3
Gross Fixed Capital Formation at Home—					
11. New buildings excluding dwellings		565	940	375	66·4
12. Plant, machinery and vehicles		1,169	1,720	551	47·1
13. Total excluding dwellings		1,734	2,660	926	53·4
14. Dwellings		652	560	−92	−14·1
15. *Additions to Stocks and Work in Progress*		49	256	207	422·4
16. *Exports of Goods and Services—*					
(*a*) Iron and steel	137		229	92	67·1
(*b*) Ships and boats	51		62	11	21·6
(*c*) Other products of metals and engineering industries	1,165		1,853	688	59·0
(*d*) Other U.K. merchandise	1,321		1,690	369	27·9
(*e*) Re-exports	101		101	—	—
(*f*) Services	788		860	72	9·1
	3,563				
Net tax on exports	−84		−112		
Timing and other adjustments	+45	3,524	4,683	1,159	32·9
17. Total disposals		19,267	24,208	4,941	25·64

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TABLE 4B

Current Foreign Transactions

Income	1954 £m.	1962 £m.
18. Exports of goods and services (factor cost)	3,524	4,683
Taxes incorporated in realized price of above exports	84	112
19. Net property income from abroad	225	267
Total	3,833	5,062

Expenditure	1954 £m.	1962 £m.
20. Imports of goods and services	3,596	4,692
21. Net governmental and private gifts, migrants' remittances, pensions, etc.	9	70
22. Net acquisition of foreign capital assets or discharge of liabilities (balance of current payments)	228	300
Total	3,833	5,062

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1962 figures in the tables have been arrived at.* We shall deal only with the broadest interrelationships, without detailing the manner in which some of the aggregates have in fact been made up sector by sector.

36. On the supplies side, the output of the "index of production" industries (item 1) and that of other forms of economic activity (item 2) may be taken as the starting point. How the increase in output might be divided between them depends on the shape of demand on the other side of the account. A provisional non-industrial expansion corresponding to the key figure adopted for industry may be estimated in the light of past experience, which shows in this country a concentration on expansion of industry rather than other activities, and checked and adjusted when the details on the demand side are worked out. The resulting increase in gross product of $2\frac{3}{4}$ per cent. annually (item 3) might equally be taken as the key starting figure.

37. Imports of goods and services similarly depend on the shape of demand, but may be provisionally estimated from their past relation with total supplies and disposals, as Fig. 3 shows. With the imports so estimated, these can be included in Table 4B. Thence with independent estimates of net property income (item 19) and the balance of current payments aimed at (item 21), we reach a figure of required exports of goods and services. This brings us back to the right-hand side of the supplies and disposals table at item 16. Within this item separate independent estimates were made of the likely export performance of the non-metal-using sectors, for iron and steel and for ships. Since little was expected from expansion of textile or coal exports†, a large increase was thrown upon exports of the products of the metal-using industries.

38. On moving up the table we make allowance for additions to stocks and work in progress (item 15) which were abnormally low in 1954 and for investment in new dwellings (item 14) which reached record proportions in 1954 and is now estimated as continuing at 1956–57 rates. An independent estimate is also made of Public Authorities' current expenditure on goods and services. Such expenditure has been practically constant in real terms since 1952. With the possibility of some minor economies in defence, only a modest increase in the total seems appropriate. The crucial item, gross fixed capital formation other than dwellings, has been estimated partly from the plans or intentions of the publicly owned industries, and partly from a consideration of the available records and estimates of investment and increases of manufacturing output in the past.

39. There remain the rest of supplies, which are allocated to the remaining item, consumers' expenditure. Perhaps fortunately, the rate of increase of consumers' expenditure is on this basis about equal to that of gross domestic product. Within consumers' expenditure, larger proportionate increases are assigned to durable goods and vehicles, these having been favoured in the past. The disposition of the supplies being now set

* A word is necessary on the status of the figures now printed. The original exercise upon which the Iron and Steel Board's Development Plan was based was conducted by an official working party, in which the authors of this paper took part, and for which a number of possible models of the national economy in 1962 were prepared by the staff of the Central Statistical Office and the Treasury. For the model adopted as the basis of *Development in the Iron and Steel Industry, Special Report* 1957 the responsibility lies with the Iron and Steel Board. Substantially the same model is presented here. Since the original work was done, however, the basic figures for 1954 have been revised, and it has been thought best to conform in this paper to the revised figures—that is to those published in "National Income and Expenditure, 1957", as well as to make certain minor amendments, in the light of more recent information, to the 1954 distribution of steel consumption given in the Iron and Steel Board's report and to revise some of the elements in the 1962 economic accounts. These changes, and any errors or improbabilities they may introduce, are the personal responsibility of the authors. They affect the total steel estimates by less than one per cent.

† See, however, the remark of Mr. Schumacher in the discussion.

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out, item 4, the necessary imports, could be re-estimated in detail. This has been done on the assumption that no exceptional measures are taken by way of forcing substitution of home-produced for imported materials or curbing the freedom now accorded to consumers' tastes. The resulting correction to imports was trivial, and was carried through the accounts to give the results shown.

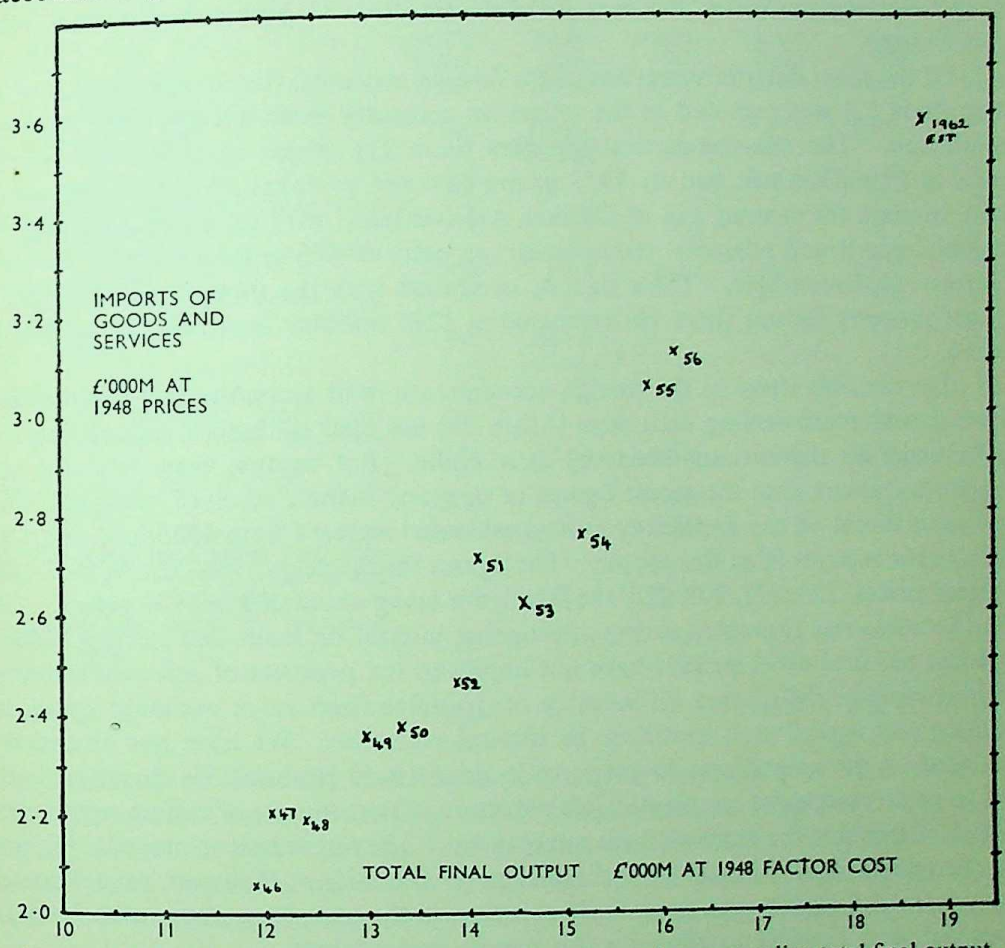

FIG. 3.—Relation between imports of goods and services and corresponding total final output. Final output = Consumers' expenditure plus Public Authorities' current expenditure on goods and services plus investment plus exports = Gross Domestic Product plus imports.

40. By the balancing of the two accounts, two items, it has been stated, have been thrown up as residual: consumers' expenditure, for which the result at first glance is not disquieting, and on the other hand exports and within them metal-goods exports, where the indicated increase is large indeed—59 per cent. between 1954 and 1962, or an average (compound) of 6·0 per cent. annually.

The Foreign Transactions Account

41. A few words are due on the mechanisms that indicate this large export requirement attaching to a rise of 3·75 per cent. a year in industrial production or 2·8 per cent. in gross domestic product. The crucial items are in the foreign transactions account. First, imports of goods and services do not from Fig. 3 seem likely to depend greatly on the form

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taken by the additional output. By the use of average input-output coefficients based on Tables 18 and 19 of *National Income and Expenditure*, 1956, representing the import content of the various items on the right-hand side of the account, an increase of 28½ per cent. in imports of goods and services is indicated, against the 30½ per cent. in the table. But such a calculation will give too low a result, for marginal raw materials, marginal food and clothing purchases and marginal fuel are likely to have a higher than average import content.

42. Of the other determining items in the foreign accounts, the current surplus of £300 million (item 22) was regarded as the minimum necessary in an average year, in view of commitments. The non-commercial transfers (item 21), which in 1954 were nearly in balance at £9 million net, had by 1957 grown to a net payment of £77 million (current prices), through the running out of Defence Aid combined with increased outgoings such as colonial grants and migrants' remittances: an entry of £70 million for 1962 allows for no further expansion here. These figures, combined with the import requirements and with net property income (item 19) estimated at £267 million, determine the export level adopted.

43. The invisible items in the foreign accounts are most recondite and unpredictable; and we should avoid dealing with them if they did not react with such an uncomfortably large leverage on the national economy as a whole. But because they do so, it seems necessary to remark that the recent figures of property income received and paid abroad throw some doubt on the apparently modest estimated increase from £225 million to £267 million in net receipts from this source. The figures for the years 1954–57 are, in £ million at current prices, 225, 177, 203, 226, the last figure being about 200 in 1954 prices. There should be some rise through a saving of outgoing interest on loans and sterling balances; but recent political developments have not improved the prospect of safe or easy returns on investments in the oversea oil industry or (to judge from stock exchange quotations) in mining and agricultural operations in tropical countries. We have not amended the table: but as the amendment to property income would probably be downward, there seems to be no margin for any appreciable reduction of the estimate of export requirements.

44. It is true that the terms of trade might move. They have indeed moved eight points in our favour during 1957, with a benefit of some £300 million. However, we are postulating an increase of exports at a more rapid rate than in the past, on a competitive basis, and it does not seem possible to allow for this benefit in the future.

IV. The 1962 Economy in Terms of Steel

45. The next step is to apply the 1954–62 increases in Table 4A to the sector-by-sector framework of Table 5 which is relevant in estimating steel consumption. This presents certain complexities. The heavy steel-using sectors of shipbuilding and rolling stock for home railways for which direct estimates have been made are not expected to show the rapid growth of other investment sectors. They are included in the broad investment item 12 of Table 4A; on their removal, the increase in the remaining items is found to be 53·2 per cent.

46. Passenger cars also involve a special and complex calculation, as consumers' expenditure on vehicles (new and secondhand) and business investment in new vehicles are both involved. Finally there is considerable difficulty in treating the large aggregate of steel consumption entered in Table 5 at item 7 (*f*): "Intermediate supplies and ad-

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TABLE 5

Projection of Steel Consumption

	1954 *Estimated Consumption* ('000 *Tons*)	*Rate of Increase Indicated in Economic Model* (*Per Cent.*)	1962 *Consumption*			*Net rate of Increase* (*Per Cent.*)
			In Proportion ('000 *Tons*)	*Adjustments* ('000 *Tons*)	*Total* ('000 *Tons*)	
1. Merchant shipbuilding, ship repair and marine engineering	960	Direct estimate	..	..	1,105	15·1
2. Coal mining	638	..	..	..	848	32·9
3. Home railways track and rolling stock	870	..	..	..	790	9·2
4. Fixed investment in dwellings	154	−14·1	132	—	132	−14·1
5. Fixed investment in other new buildings and works	1,300	66·4	2,163	−187	1,976	52·0
6. Building repair and maintenance	70	..	..	..	90	28·6
7. Other metal-using industries' output:						
(*a*) Defence	580	Direct estimate	..	..	563	−2·9
(*b*), (*c*) Fixed investment in plant and machinery, road goods vehicles and public road passenger vehicles	1,975	53·2	3,026	−100	2,926	48·2
(*d*) Passenger cars for the home market	430	85·0	795	+17	812	88·8
(*e*) Exports of engineering products	2,450	59·0	3,897	+54	3,951	61·3
(*f*) Intermediate supplies and additions to work in progress and stocks of finished goods	1,970	25·4	2,471	+20	2,491	26·4
(*g*) Consumer goods for the home market	465	59·6	742	+15	757	62·8
8. Other Steel Users	580	36·0	789	—	789	36·0
Total	12,442	..	..	−181	17,230	38·5

ditions (within the metal-using industries) to work in progress and stocks of finished goods". A very uncertain attribution of the final uses of the intermediate output of the metal-using industries, derived in part from the 1950 input-output coefficients, is given in Table 6. Later and more detailed input-output coefficients should permit improvement of these figures. It is interesting, incidentally, to notice that the element of steel for consumers'

TABLE 6

Attribution of Steel in Intermediate Output of the Metal-using Industries, Including Increases in Stocks and Work in Progress

	1954 ('000 *Tons*)	*Increase to* 1962 (*Per Cent.*)	1962 ('000 *Tons*)
Consumers' expenditure	1,080	21	1,310
Public authorities' current expenditure	140	9	153
Fixed investment (largely building)	360	22	440
Investment in stocks of finished goods and work in progress (in metal goods industries and elsewhere)	160*	81*	290*
Exports	230	21	278
Total	1,970	25	2,471

* Especially uncertain figures. The 1954 figure was probably abnormally low, but not to the extent of stocks and work in progress generally.

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TABLE 7

A Comparison of Actual Consumption of Finished Steel 1950–56 *with Consumption Calculated from National Expenditure*

(*Steel in Thousand Tons*)

	1950	1951	1952	1953	1954	1955	1956	1957	*Source of Expenditure Data*
1. *Merchant Shipbuilding, Ship Repair and Marine Engineering*—Estimated steel consumption . .	891	870	860	944	960	891	901	923	..
2. *Coal Mining*—Recorded steel consumption . .	560	562	514	578	638	656	684	747	..
3. *Railways* (excluding exports of track material and rolling stock)—Estimated steel consumption .	680	750	660	820	870	910	970	1,080	..
4. *Fixed Investment in Dwellings* at constant prices (£m. at 1948 prices)	323	316	377	493	510	459	448	439	C.S.O. Monthly
Steel .	98	95	114	149	154	139	135	133	Digest.
5. *Fixed Investment in Other New Buildings and Works* at constant prices (£m. at 1948 prices) . .	463	452	460	483	532	598	661	692	C.S.O. Monthly
Steel .	1,131	1,104	1,124	1,180	1,300	1,461	1,615	1,691	Digest.
6. *Building Repair and Maintenance Work* (£m. at 1949 prices)	470	442	434	454	468	457	469	460	Annual Blue
Steel .	70	66	65	68	70	68	70	69	Book.
7. *Other Metal-using Industries*—									
(*a*) Defence—expenditure at constant prices (£m at 1955 factory prices)	249	386	571	640	645	630	606	568	Economic Survey.
Steel .	173	269	513	575	580	566	545	511	
(*b*) Fixed investment in plant and machinery at constant prices (£m. at 1948 prices) . .	602	659	624	632	699	769	778	840	C.S.O. Monthly
Steel .	1,447	1,584	1,500	1,519	1,680	1,848	1,870	2,019	Digest.

TABLE 7—(*continued*)

(*c*) Gross fixed capital formation in road goods and public road passenger vehicles (£m. at 1948 prices)	103	84	75	83	95	116	118	112	Annual Blue Book.
Steel	320	261	233	258	295	360	366	379	
(*d*) Production of passenger cars for home market (£m. at 1955 factory prices)	57	61	87	131	180	270	210	228	Economic Survey.
Steel	136	146	208	313	430	645	502	545	
(*e*) Exports of engineering products—volume index (1954 = 100)	101	102	102	98	100	109	114	118	Board of Trade.
Steel	2,475	2,499	2,499	2,401	2,450	2,671	2,793	2,891	
(*f*) Intermediate uses—production of components etc. for other industries (£m. at 1955 factory prices) (includes increases in work in progress and finished goods within the metal-using industries)	708	761	721	700	810	860	717	647	Economic Survey.
Steel	1,722	1,851	1,754	1,702	1,970	2,092	1,744	1,574	
(*g*) Metal consumer goods for the home market—output (£m. at 1955 factory prices)	271	297	271	308	355	410	343	396	Economic Survey.
Steel	355	389	355	403	465	537	449	519	
8. *Other Steel Users*—									
C.S.O. Index of production for non-engineering manufacturing industry (1948 = 100)	117·0	119·1	109·8	121·5	130·2	134·4	135·2	137·0	C.S.O. Monthly Digest.
Steel	521	531	489	541	580	599	602	610	
9. *Total Calculated Steel Equivalent* based on 1954 actual consumption (thousand tons)	10,579	10,977	10,888	11,451	12,442	13,443	13,246	13,691	..
10. Actual steel consumption (thousand tons)	10,630	11,003	10,911	11,404	12,442	13,221	13,570	13,626	..
11. Actual less calculated, as percentage of calculated	0·48	0·24	0·21	−0·41	(0)	−1·68	+2·39	−0·48	..

N.B.—In items 4 onwards, the "Steel" figures must not be regarded as separate sectional estimates, but merely the result of applying the economic indicators to the basic steel weighting of 1954.

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expenditure in this table, perhaps rather over a million tons, exceeds considerably that shown in Table 5 as going into consumer goods supplied directly by the metal-using industries, that is 465,000 tons plus about half the steel for home-market cars. This intermediate output for consumers would include the considerable tonnage of tinplate used by can and box makers and sold to other industries for canning or packing goods for home consumers.

47. The result of applying the several rates of increase to the various sectors is shown in Table 5. The adjustment column is included with the intention of allowing for possible changes in the ratio of steel usage to output within the sectors. Their amounts are inevitably speculative. Economy in industrial building seems pretty certain. For investment in plant and machinery the nationalized industries expected the steel content of their programmes to fall rather behind the value increase. Other adjustments come from an expectation of extra uses of steel plate and sheet, the latter especially in motor vehicles, and from allowance for the fact that tinplate was scarce in 1954.

48. Table 7 shows that over the period 1950 to 1956 the calculated steel consumption content of national output moved almost exactly in line with the known consumption of steel. The calculated content was arrived at on the basis that the steel used in each sector varied directly with the volume of output of that sector. In the 1962 estimate, however, this proportionality has been departed from to the extent of the estimated net saving of steel of 181,000 tons shown in Table 5. It is fully recognized that Table 7 is only supporting evidence for the view that it is broadly reasonable to assume on average that steel consumption is proportionate to the output of the sectors. Within sectors there have certainly been movements, some one way and some the other; this is stressed by the note at the foot of the table. A much fuller treatment of this important question is desirable and it is for reasons of space alone that it is not further examined here.

49. The total estimated steel consumption in 1962 comes to 17·23 million tons of finished steel,* or about 23½ million ingot tons. This is an average increase from 1954 of 4·15 per cent. annually, compared with the increase of 3·75 per cent. annually for industrial production. The elasticity or ratio of these growth rates, is 1·11.

The home purchases, as distinct from consumption, of steel would be higher, since in a year of normal growth stocks would be expected to increase by some 300,000 to 350,000 ingot tons. This gives the 24 million ingot tons of home demand used in the Iron and Steel Board's Development Report. Export demand was estimated at 5 million ingot tons; and to provide that margin for contingencies which has been lacking in post-war years, the Iron and Steel Board recommended that a capacity of 29 million tons should be aimed at by 1962. This, with a small tonnage of imports and re-usable steel, would make it possible to meet, if necessary, a total demand of 30 million tons.

The Steel-intensive Economy

50. We may now briefly recapitulate how it happens that the economic expansion envisaged requires that industry as a whole become more steel-intensive. Within consumers' expenditure, the comparatively heavier steel-using elements of vehicles and durable goods are expected to take an increasing share. Fixed investment—except in housing, a relatively light steel-user—is expected to absorb an increasing share of national product. And a rapid growth of metal-using exports is postulated.

* The figure published in the Iron and Steel Board's Development Report was 17·36 million tons. The difference is due to the minor revisions referred to in the footnote to paragraph 35.

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51. To account for the fact that a change in the relations of steel consumption and industrial production is implied in comparison with the years *before* 1954 we may refer to Table 8. The point to note is that the disparity between the first line and those following is greater in the estimates for the future than it was in the past. (Vehicles constitute an exception but the quantity of steel involved is much less than that in the other items.)

TABLE 8

Past and Future Rates of Increase

	1950–54		*Allowed for*, 1954–62	
	Percentage Increase	*Annual Rate*	*Percentage Increase*	*Annual Rate*
Total industrial production	14·03	3·34	34·2	3·75
Fixed investment in plant, machinery and vehicles	11·22	2·68	47·1	4·9
Fixed investment in "other new buildings and works"	16·47	3·82	66·4	6·5
Consumers' expenditure on—				
Durable goods	20·40	4·75	59·6	6·0
Vehicles	135·44	21·63	98·0	8·9
Exports of engineering products (excluding ships)	−1	−0·25	59·0	5·96

52. The implied structural changes are large. A comparison of the first and last columns of Table 9 shows how they alter the disposition of total expenditure. The extent to which the changes have in fact developed in the last few years will be indicated in the next section. If we look for a moment beyond 1962 we see that these changes cannot be expected to continue in the same way indefinitely. In particular, the proportion of national income devoted to fixed investment must be subject to some limit. The post-war trends in this country, the example of other countries, the plans of industry in both public and private sectors, the need for transport improvements and the pace of technical advance all point to the need, and, rather less certainly, the likelihood of an expanded proportion of national income being devoted to fixed investment; but this does not mean a continually expanding proportion. How high the proportion might need to be to sustain steady growth of the national product at a few per cent. annually we cannot tell, but one may hope that it will settle around the value appropriate to such growth. If the proportion remained a constant instead of a rising proportion of national income, the elasticity of steel consumption with respect to industrial production would assume a lower value. Thus the period for which we are catering is viewed as a period of transition towards a pattern that will provide the means of growth and solvency. If and when that pattern is reached the increase in steel-intensiveness may cease, as it appears to have done in the United States.

53. An important qualification attaches to the suggested structural changes. The type of rearrangement shown in Table 9 over 1954–62 could not take place quickly. Public authorities' current expenditure is normally comparatively stable and can seldom be decreased in absolute terms. So a *quick* increase in investment, even if there were no problem of the different forms of goods and services being needed, cannot readily be provided by direct transfer mainly from public authorities' expenditure in the manner suggested by the last column. Yet because of the time taken first to complete and then to

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TABLE 9

Disposition of National Income (Factor Cost)

Percentages

	1954	1955	1956	1957	1962
Consumers' expenditure	64·2	64·3	62·5	61·9	63·7
Public authorities' current expenditure on goods and services, including defence	19·0	18·4	18·5	17·9	16·7
Gross domestic fixed capital formation:					
Excluding dwellings	11·2	12·3	12·8	13·5	13·8
New dwellings	3·8	3·4	3·2	3·0	2·6
Additions to stock, etc.	·3	1·9	1·3	2·1	1·3
Investment abroad or discharge of foreign liabilities (the current foreign balance) plus net governmental and personal gifts, etc., to abroad	1·5	−0·3	1·7	1·6	1·9
Total National Income and Depreciation	100	100	100	100	100

secure results from investment, it is desirable that the increase to a higher level should take place as quickly as possible and that this increase should be maintained. Thus, although the table superficially gives a reassuring picture of consumers' expenditure rising over the 8-year period by the same proportion as national income, it does not alter the fact that for an early substantial rise in investment, there would seem to be no alternative—apart from temporary and probably embarrassing raids on the foreign balance—to finding the resources for investment by temporary restraint in private consumption.

V. EXPERIENCE IN 1954–57

54. The percentages shown for 1955–57 in Table 9 illustrate the point made at the end of the last section. The share of public current expenditure on goods and services has diminished as expected—indeed faster—but by no means enough to compensate for the strong increase in the share of investment other than dwellings. In 1955 this strong increase (including a large increase in stocks) was in effect financed largely by a deficit on foreign account, with extremely inconvenient results. There was in 1956–57 a reaction. This was brought about to a considerable measure by a re-arrangement of the type suggested above as initially appropriate, so that consumers' expenditure gave way to investment and an improved foreign balance. The foreign trade balance itself was then reasonably satisfactory in 1956 and 1957. But inflation continued and this, with an associated run on the sterling reserve led in the autumn of 1957 to new measures of restriction. These took the form of the 7 per cent. Bank Rate, credit restriction and a limit on planned increases in public investment. They thus operated primarily to check investment rather than consumption. This is not the place to discuss alternative methods of dealing with inflation. But if these particular methods have to be continued for long, they may jeopardise the promising pattern of high investment recently achieved and lead to a significant departure from the assumptions on which the steel plan is based.

55. Some details of the progress made to the end of 1957 are given in the eight graphs of Fig. 4. For comparison with the assumptions made in the 1962 model of Tables 4A and 4B there are included lines showing a regular progression between 1954–62, though an assumption of regular progression would in some cases always have been unrealistic. These graphs speak for themselves. The vital export items had up to the end of 1957 risen much as suggested. The other vital elements in the field of fixed investment were

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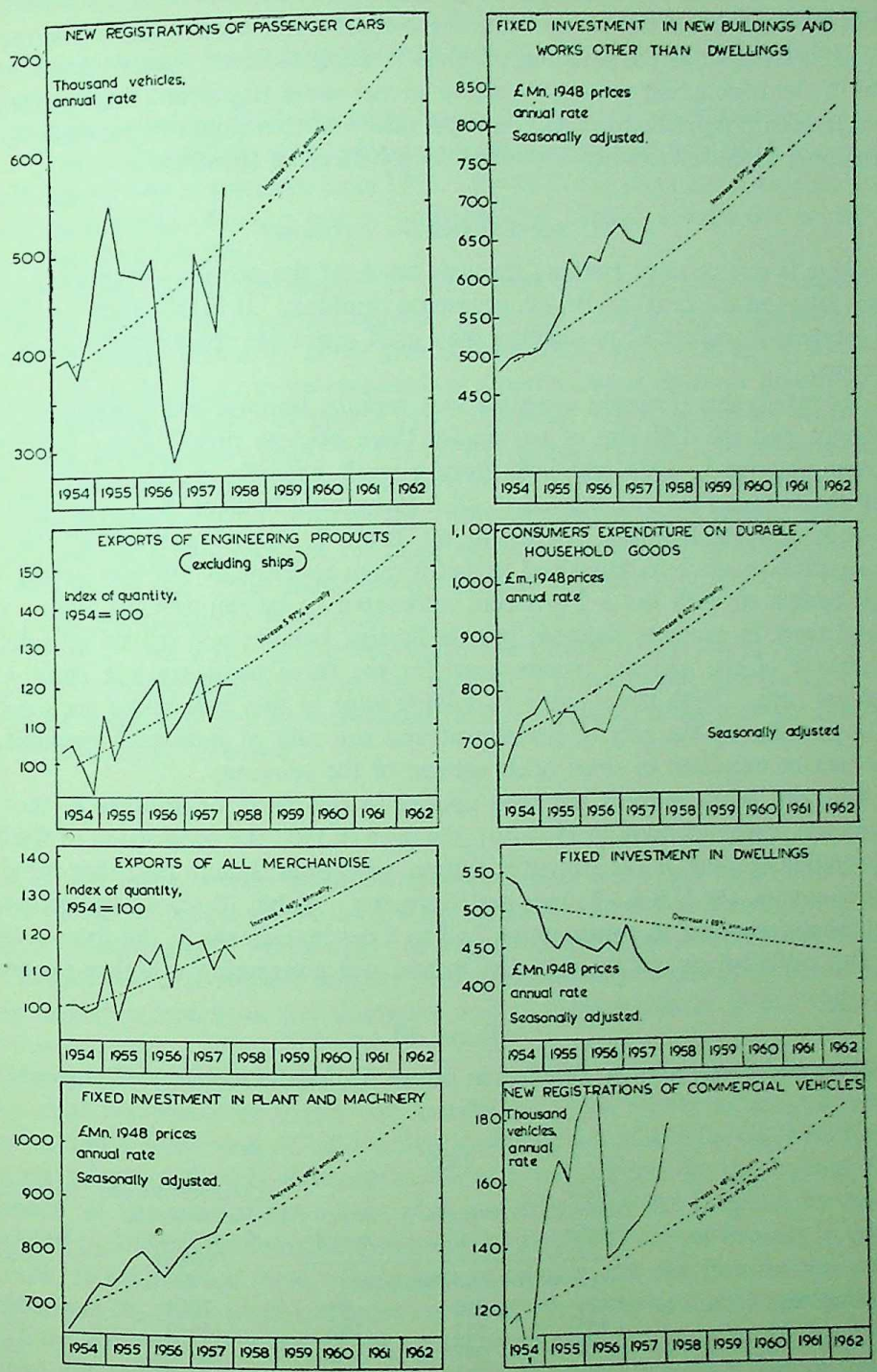

FIG. 4.—Eight main economic indicators, 1954 to 1957–8, and their rates of increase suggested in Tables 4 and 5.

The vertical scale, if expressed as an index, 1954 = 100, is the same in each diagram.

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still well above the trend-lines, though the great spurt of 1955 had been moderated. Since the end of 1957 the situation has become, in the short term at least, more uncertain. Expansion has ceased while the inflation problem is being tackled. Some observations will be made in the concluding section on a few of the more important implications of this whole exercise in prognostication and on some measures that might assist the development of the national economy towards expansion in a pattern of solvency.

VI. AN EXPANDING ECONOMY

56. There is one concept running through much of the previous discussion—and this concept is often at the heart of British economic thinking. It is the belief—or fear—that a rapid increase in industrial production may itself aggravate the problem of the balance of payments.

57. The fear is that if output increases very rapidly, imports will increase at least correspondingly, and the difficulty of paying for them become prohibitive. It is true that the economy started in 1954 and 1955 from a weak position in this respect. In 1954, although the foreign accounts showed in all a favourable balance, this was facilitated by exceptionally low stock-build. As shown by Table 9, the proportion of the national income devoted to stock-building and to net foreign investment and net grants was 1·8 per cent. compared with the 3·2 per cent. estimated to be required in 1962. In 1955, stocks and work in progress went up, but the foreign balance was driven into deficit and the percentage of the national income used for the three purposes just mentioned was only 1·6 per cent. With the country on a knife edge of this sort, there are clearly difficulties in pushing up the rate of investment and the rate of industrial progress, unless restraint can be exercised in some other section of the economy.

58. Restraint on investment has been applied in one form or another in most of the post-war years, except perhaps in 1955–56; but even in 1955 the proportion of fixed investment, excluding housing, in the United Kingdom, was lower than in the other three leading Western countries, the U.S.A., France and Germany. Table 10 shows the proportion of national income devoted to consumption and to fixed investment in the four countries in 1955. This table brings out the fact that Britain was consuming a higher proportion of

TABLE 10

Consumption and Investment in Relation to Gross National Product at European Relative Prices

1955

	Gross National Product	*Consumption**	*Investment†*	*Consumption as Percentage of Gross National Product*	*Investment as Percentage of Gross National Product*
	(£ *per Head of Population*)			*Per Cent.*	*Per Cent.*
United Kingdom	411	290	50	70·4	12·1
U.S.A.	825	552	132	66·9	16·0
France	346	241	48	69·8	13·9
Germany	348	221	53	63·4	15·1

* Consumption includes health, education and other publicly financed consumption.
† Fixed investment excluding residential construction.
Expenditure on defence and general government administration are excluded under both these heads.
Source: Milton Gilbert and Associates (1958), which should be consulted for details of the definitions.

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its national product and investing a lower proportion than any of the other major Western countries. Similar figures for the Eastern European countries would show that they were consuming an even lower proportion of their national income and investing a higher proportion.

59. The comparison in 1955 was more favourable to the United Kingdom than it had been previously. In 1950 the U.K. investment proportion was only 10·6 per cent., with the other countries about the same as in 1955. By 1957, the U.K. had increased its investment proportion rather further, but is probably still behind though not so far behind the other Western countries.

60. Two points should be made. It takes time for investment to materialize and yield its gains; the British effort has been developing favourably and if maintained should progressively give good results. If the greater investment proportion of the last few years could be maintained—and not be regarded as a temporary investment boom only—then undoubtedly important gains in productivity and total output would be made possible. The Steel Plan is specifically based on the 1957 investment proportion being maintained.

61. The second point to note is the observation by Barna (1958) that a larger proportion of the recent higher British investment has been directed towards further production of consumer goods than to improving the basic means of production, in comparison at least with Western Germany. This reinforces the dangers to future productivity growth of any cut-back in basic investment in Britain.

62. It is of course abbreviating drastically the discussion of the growth of national output and the productivity advances on which it is based to concentrate on investment and to ignore social and educational factors that undoubtedly make a major contribution, but this limit must be imposed.

Productivity

63. Are there any measures in the narrower economic field which would tend to improve the rate of productivity increase in the United Kingdom and at the same time not prejudice the balance of payments? Many might be suggested, but three will be briefly touched on here. These have been chosen because they all have some bearing on practices which are more largely adopted on the Continent and in some instances in the United States than in this country.

64. The first is the possibility of a direct attempt to curtail consumption and release more national resources for export or investment. This would be in contrast to the measures adopted at the end of 1957 which inevitably fell selectively on investment.

65. One approach in this direction is the plan put forward by the Labour Party for an extension of superannuation. This plan has at its core the proposal to cut current consumption by increasing contributions from workers of all classes broadly in proportion to income. It proposes to divert the surpluses coming into the fund in the early years into investment in order to give a more rapid growth of the economy and the ability to pay pensions later on. It is one thing, however, to enunciate policies of this sort. It is another to get them carried through. But it is at least interesting that in all political parties there is thought of curtailing present consumption to increase the volume of investment and the rate of progress of the economy.

66. Second, there are features of the British cost structure, as determined by national

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policy, which result in less incentive to increase productivity and to invest than is found in Western Europe and the U.S.A. One striking difference is the form in which employers feel the incidence of social service costs. In France and Germany, to take the steel industry as an example, the net wage received by workers is below that in Britain, after deduction of the contribution they make to social security schemes. When however the employers' and employees' contribution is added to the net cash wage paid by the employer, the gross employment cost per employee is higher in both Germany and France than in the United Kingdom. In contrast, the contribution paid by the Government from its general taxation is much lower on the Continent than in the United Kingdom. The result is that on the Continent the cost of employing labour is more nearly equal to the total income from all sources that the worker enjoys than is the case in the United Kingdom. Here the cost of employing labour is substantially less than the income claim of labour.

67. It might well speed up the rate of productivity increase if the Continental practice were followed more closely and a greater part of the funds for the social services were collected from the employers by way of a charge on employment with a corresponding reduction in the general budget.

68. Any such scheme would require careful thought if an increase in the general level of prices is to be avoided. But an adjustment of the costs falling on industry, if it can be made, which would increase labour costs but reduce some of the elements involved in capital costs, should lead to an increase in productivity. This issue is one of the proper use of the budget as a means of allocating the national on-costs. It has nothing to do with any attempt to increase productivity by anticipatory increases in consumers' incomes.

69. Third, there is another way in which the British cost structure may be biased against productivity increases. Or to put the point more directly, there is another way in which the cost of labour may be artificially depressed in relation to plant or capital costs. This arises from the apparent angling of the price structure of the publicly-owned industries in favour of domestic as against industrial consumers—at least in comparison with the practice on the Continent. This is seen, for example, in the case of rail charges. The British basic second class passenger rate is 2*d.* a mile, the German 2·5*d.* a mile. The average receipts of course vary with special concessionary fares, but these figures roughly illustrate the relative charge to passengers. In contrast, the charges to industry per hundred miles of haul of industrial materials such as iron ore, scrap and steel all appear to be higher than the Continental rates. This is shown in Table 11.

70. Other examples of differentiation are seen in the charging for electricity and gas in the United Kingdom. The average selling price of gas and electricity to the domestic user is very substantially below the German price, but to industry the gas and electricity price here is higher.

71. In all these cases the ratio of the price paid by the domestic consumer to that paid by industry is in the United Kingdom from 1½ to 2½ times more favourable to the domestic consumer than it is on the Continent, and correspondingly less favourable to industry. These figures are, of course, only a broad indication of the position. It is difficult to know to what extent a more thorough examination would confirm the indication they give, and also to what extent they reflect real differences of cost. They do at least suggest that costs to consumers are being kept down in the United Kingdom at the expense of industrial costs.

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72. If the policy described were changed and prices to the domestic user advanced and to the industrial user reduced to give the same total proceeds, there would undoubtedly be a significant increase in the cost of living and no doubt claims for higher wages. If these claims were met to the extent of leaving real incomes as before, the wage level in Britain would be increased but the final price of manufactured goods should not be altered. The increase in the wage cost paid by industry should broadly balance the reduction in material cost. The final outcome of such a change would be an increase in labour cost in relation to the cost of industrial products generally, including new plant and equipment. There would then be an increasing tendency to replace labour by equipment and to speed up productivity advances.

TABLE 11

Illustrative Examples

Domestic and Industrial Prices—U.K. and E.C.S.C.

	U.K.			*E.C.S.C.*			
	Domestic	*Industrial*	*Ratio D/I (U.K.)*	*Domestic*	*Industrial*	*Ratio D/I (E.)*	*Ratio D/I(U.K./E.)*
Gas* . . .	19·9	11·4	1·75	27·8	8·9	3·13	0·56
Electricity† . .	1·6	1·3	1·23	3·6	1·2	3·00	0·41
Transport‡ . .	2·0	2·7	0·74	2·5	2·3	1·09	0·68

* Prices charged per therm for the U.K. by the East Midlands Gas Board; for E.C.S.C. in the Ruhr.
† Pence per kW. hour; for E.C.S.C., Germany.
‡ Pence per passenger mile and pence per ton mile; industrial rates are an average of the following:

Rate per Mile

	Miles	*Continent** (*d.*)	*United Kingdom* (*d.*)
Iron ore	100/150	1·5	1·6
Steel scrap	200/225	2·5	3·1
Steel	200/300	3·0	3·5
Unweighted average		2·3	2·7

* The E.C.S.C. publication *Europe's First Common Market* quotes as examples of through rates for similar distances iron ore 1·2, scrap 2·2 and steel 2·7 pence per mile. These may be below the average rate payable.

73. With the present fall in prices of imported materials there may now be an opportunity to make the changes here outlined. The increase in domestic prices which would arise from adjusting the publicly controlled prices in favour of industry and against the domestic buyer might be offset by the fall in import prices so that there would be no increase in the cost of living. If as a consequence wage increases were avoided or kept from exceeding the increase in national productivity, industrial prices should fall under the double impact of falling imported commodity prices and a reduction to industry of the internal publicly controlled prices.

74. In all these eventualities the ratio of wages to plant costs would then move in a sense favourable to productivity. The gains from modernization would be seen to be greater, and there would also be additional pressure towards the efficient use of labour in existing manufacturing, distribution and service organizations.

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Acknowledgments

75. This paper is based perhaps more than most on the co-operative efforts of a number of persons and organizations. While the writers accept sole responsibility for the estimates and judgments recorded here, so far as they go beyond matter already published, they acknowledge gratefully the work of the statisticians and economists in Government service who broke the ground and have been ready at all stages with technical guidance. The British Iron and Steel Federation contributed estimates and valuable criticisms in the early discussions; and essential advice was received from trade associations, public corporations and Government Departments on behalf of various steel-using industries. Finally much help has come from the staff of the Iron and Steel Board, especially from Mr. T. P. Miles, to whom Table 7 in particular is mainly due, and from Mr. L. Needleman, whose work on the national foreign accounts has permitted us some tentative judgments in that difficult field.

VII. SUMMARY OF CONCLUSIONS

76. The following is a summary of the main points of the paper.

1. The demand for steel in the 1954–62 plan is based on a 3 per cent. growth in industrial productivity in the United Kingdom. In the first three years of the plan the growth in productivity has averaged only 2 per cent. per annum.

2. The E.C.S.C. 1955–65 objectives imply approximately a 4 per cent. per annum increase in productivity. In the three years to 1957 the E.C.S.C. countries have exceeded this rate of increase.

3. The United Kingdom plan arrived at an elasticity of growth of steel consumption relative to industrial production of 1·13 (recalculated in this paper as 1·11). The E.C.S.C. assume 0·95. In the first three years of the plan the elasticity experienced in the United Kingdom is 1·3. The actual rate of consumption of steel in the United Kingdom in the second half of 1957 has been within $2\frac{1}{2}$ per cent. of the estimated trend. The slower growth in industrial productivity than expected has been balanced by a greater use of steel in relation to industrial output.

4. The historical record back to 1903 shows a long-term relation between steel consumption (S) and industrial production (P) given by the formula:—

$$S = 16{\cdot}75 \left(\frac{P - 30}{100}\right)^{0{\cdot}795}$$

This formula gives steel consumption in millions of tons, with industrial production as a hundred in 1948 and steel consumption 13·5 million tons in that year. This relationship fits the facts of steel consumption in 1954 and in other periods when there was a high level of employment.

5. The formula suggests an elasticity of approximately 1 over the period 1954–62, compared with the 1·13 implied in the United Kingdom steel plan. Steel consumption is therefore estimated at approximately 1,000,000 ingot tons more than by the historical formula.

6. The steel content of the various sectors of the economy was, on the average, taken as remaining constant in relation to the volume of output of the sectors over the period 1954–62 except for some net saving of weight in steel for construction

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and some other minor exceptions. The figures for the period 1950–57 confirm this assumption.

7. The reasons for expecting steel consumption to increase by 4·2 per cent. annually in relation to an assumed industrial production increase of 3·8 per cent. annually are that—

(*a*) engineering exports,
(*b*) investment in plant, machinery, vehicles and non-residential building, and
(*c*) consumers' expenditure on durable goods, including cars

are all expected to increase more rapidly than industrial production generally. It is thought that the increase in exports needed will require to be mainly in the form of metal goods, and that the proportion of investment in the national income reached in 1954–55 will be maintained, but that an increased share of it will be in the form of industrial investment rather than housing.

8. The volume of output of the major steel-using sectors of the economy in the first three years of the period has developed in line with the trends estimated.

9. Although the key sectors of investment and exports have till recent months shown increases at least as large as expected, industrial production generally has not increased since 1955 in accordance with expectations—of late largely through the steps taken in autumn 1957 against inflation. This sluggishness, if continued, will make it difficult to progress along the lines suggested, and in particular to maintain the high-investment pattern recently achieved.

10. A number of suggestions are made as to policies which might assist the growth of productivity and thus the possibility of economic expansion combined with stability. These are—

(*a*) Any moderation of the claims on resources should preferably check consumption rather than investment. A check, if needed, might take the form of diverting some part of current incomes from consumption to superannuation savings or to payment for the social services.

(*b*) To encourage a more effective use of existing plant and to reflect the real benefit of investment it is suggested that—

(i) it may be desirable to increase the employment cost and reduce the capital cost element facing the employer by changing the incidence of his contribution to the social services, and

(ii) it may be desirable to reflect a higher cost to domestic users and a lower cost to industry in the pricing of the products of the publicly controlled industries. This, through repercussions on wages, would tend to increase employment costs and reduce plant costs.

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APPENDIX A

Industrial Production and Steel Consumption in the United States

The relation between industrial production and steel consumption in the United States over the period 1899 to 1956 is shown in Fig. 5. The curve drawn in the figure has the equation

$$S = 94{\cdot}3\left(\frac{P - 10{\cdot}2}{100}\right)^{0{\cdot}785} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Where S represents steel supplies in millions of tons, ingot equivalent, and P is the index of production, 1947–49 = 100.

The equation neglects the effect of cyclical slack, but may be seen to describe the data more comprehensively when extended by the specific introduction of this notion. We imagine the existence at each moment of a notional "possible" or "full employment" rate of industrial production P_n, which the actual rate P_t attained only now and then. A value is assigned to this invented rate P_n simply by drawing a smooth curve through those points on a time-graph of P_t at which labour unemployment was low. The transition between 1929 and 1948 presented difficulties, which were resolved, with some guidance from peace-time unemployment rates in the 1930's, by omitting two years of the war period and establishing a smooth increase over the rest, regardless of the vastly increased production indices recorded for 1941–44. This procedure is arbitrary, but it was found that any plausible minor changes in P_n had little effect on the results.

It is then assumed that

$$S = A\left(\frac{P_n - P_0}{100}\right)^{\alpha} - c\,(P_n - P_t). \quad . \quad . \quad . \quad . \quad (3)$$

Where A, α, c, P_0 are constants.

The first term represents what S would have been had P_t reached the notional level P_n. The second term represents the loss of steel demand through P_t not reaching it. Approximate values of A, α, P_0 were found from the years when $P_n - P_t$ was small, and thence a suitable correction factor c for other years. Slightly different factors would have suited different periods. With $c = 1{\cdot}4$ million tons per point of the index, the logarithmic plot of $S + c(P_n - P_t)$ against $P_n - P_0$ is shown in Fig. 6. The nearly linear relation there disclosed for the pre-war years gives the values of A and α, 94·3 and 0·785, appearing in equation (2).

The post-war rates of steel consumption in the United States—and even more the war-time rates—appear to be definitely less than would be indicated by any interpretation on the above lines of the pre-war relationship. There was a shortage of steel during the war, relieved as new plant came into operation in the later forties, and there is thereafter a suggestion of a return to something like the former type of general relation between P and S, with S permanently lowered. The remarkable expansion of industrial output in

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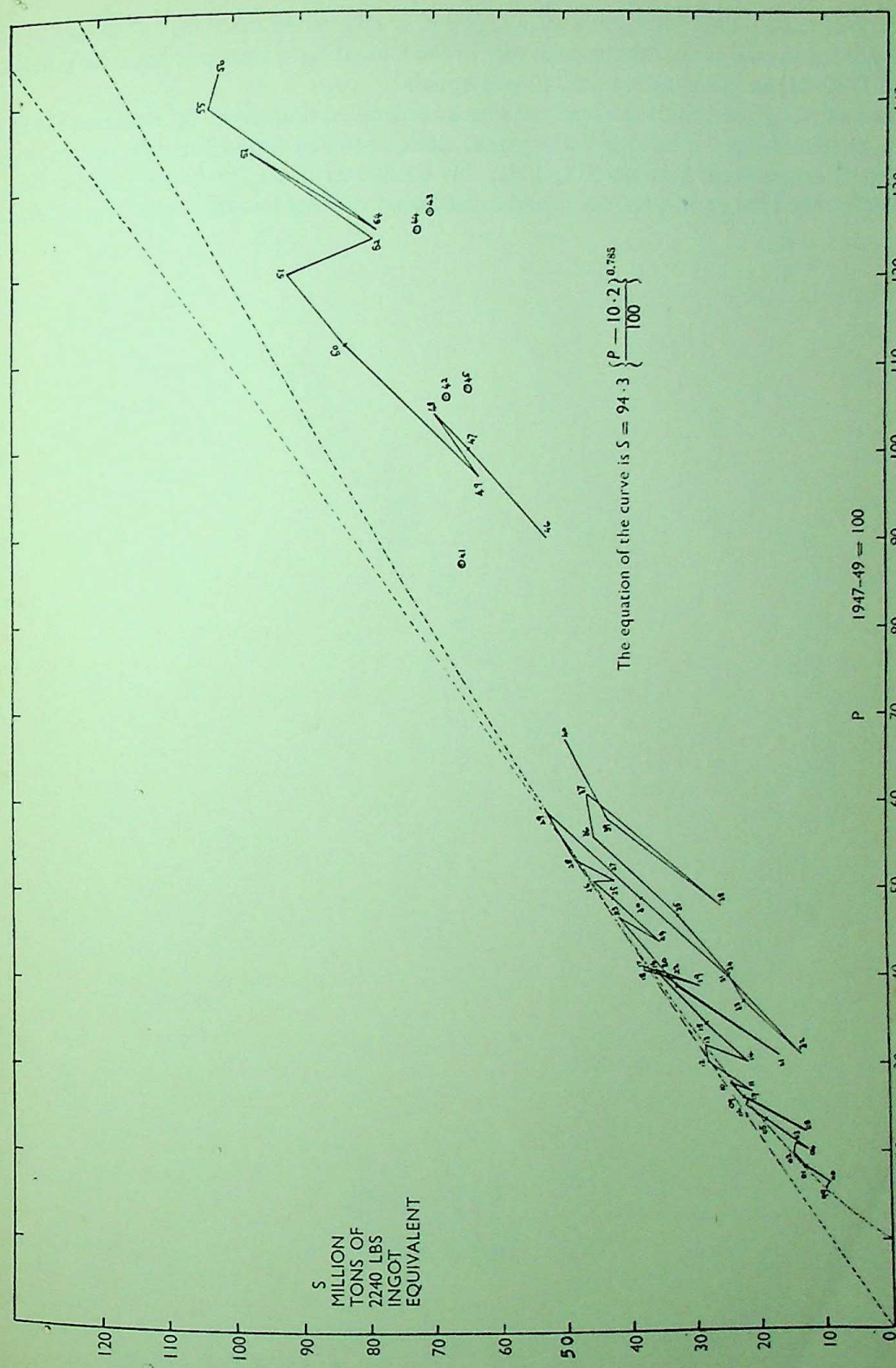

FIG. 5.—Steel supplies and industrial production, United States 1899–1956.

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the war years, with little increase in steel supplies, is interesting evidence of the extent to which American steel usage (about twice that of the United Kingdom per head of population in 1950–51) must be taken up by non-essentials.

The analysis given here is in some ways an extension of that given by Boschan (1954). With a similar device for dealing with cyclical slack, Boschan found for different periods different linear relations between S and P_n; his coefficient of P_n in the expression for S was less for the later period, as one would expect from α being less than unity in equation (3).

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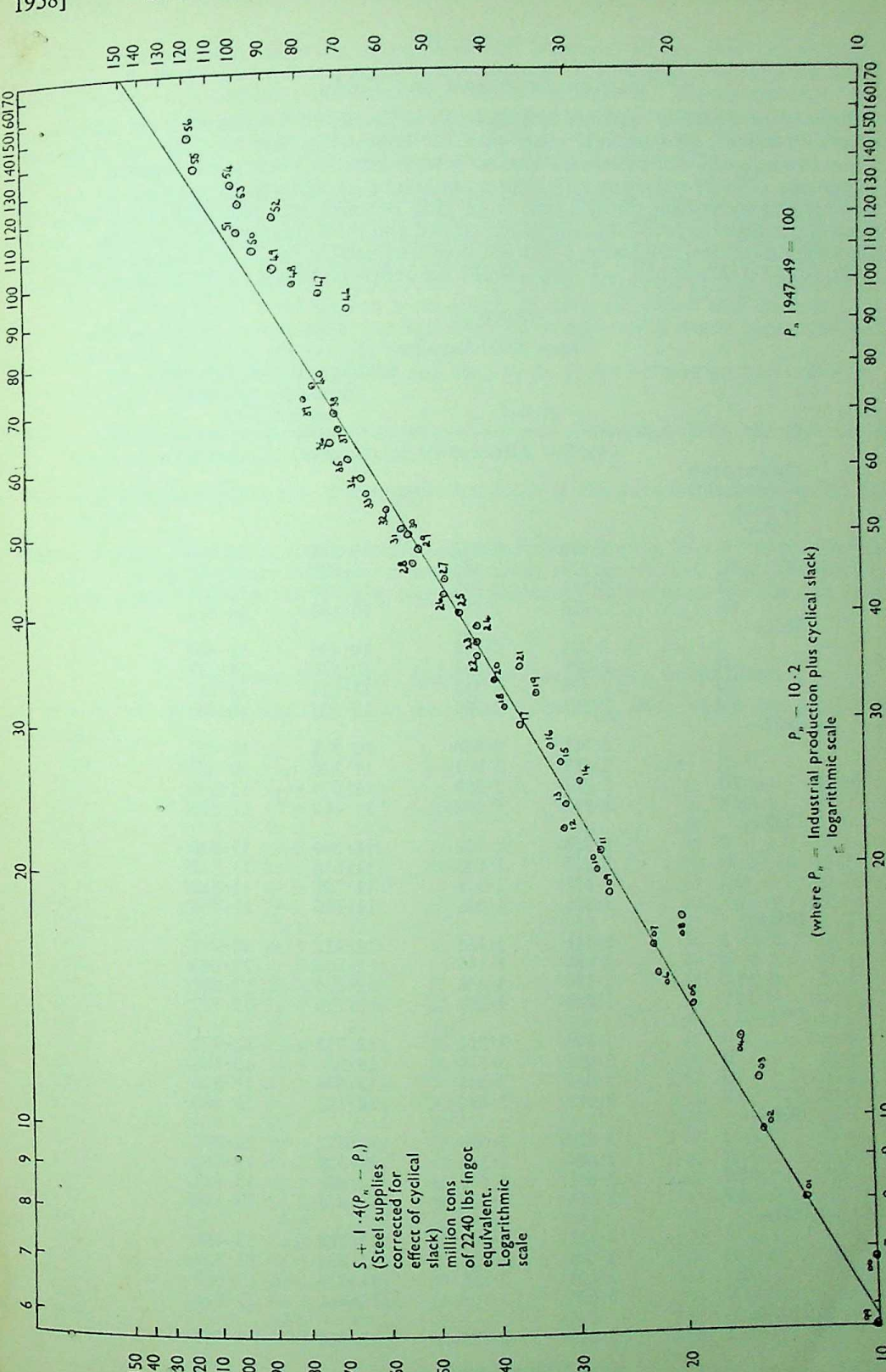

FIG. 6.—Steel supplies and industrial production, United States 1899–1956. Illustration of approximate relation:

$$S = 94{\cdot}3\left\{\frac{P - 10{\cdot}2}{100}\right\}^{0{\cdot}785} - 1{\cdot}4\,(P_n - P_1)$$

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APPENDIX B

Notes on the Graphs and Tables

Fig. 1.—Home consumption is reckoned as steelworks' deliveries of finished steel for home consumption or export, plus imports other than for conversion, less exports, less estimated increases in consumers' and merchants' stocks. A small addition is made for estimated second-hand supplies. The 13-week quarterly figures are seasonally adjusted by dividing by a set of four coefficients unchanging through time from 1954 onwards but previously showing some movement of normal consumption out of the second and third quarters into the first. Stock changes show a seasonal movement, which has been estimated as equal, in the four quarters of the year, to +2·116, −0·385, −1·463, −0·268 per cent successively of the current quarterly

TABLE 12

Home Steel Supplies

Million tons, finished weight

Quarter-years of (normally) 13 weeks	*Actual*		*Seasonally Adjusted Annual Rate*	
	Supplies	*Consumption*	*Supplies*	*Consumption*
1950—				
I	2·676	2·742	10·328	10·820
II	2·572	2·675	9·892	10·264
III	2·361	2·432	10·508	10·635
IV	2·610	2·781	10·180	10·832
1951—				
I	2·721	2·775	10·436	10·884
II	2·807	2·931	10·836	11·288
III	2·476	2·513	11·024	11·012
IV	2·767	2·784	10·804	10·844
1952—				
I	2·765	2·680	10·572	10·452
II	2·888	2·803	11·220	10·836
III	2·727	2·559	12·076	11·240
IV*	3·170*	3·062*	11·640	11·176
1953—				
I	3·060	2·920	11·640	11·320
II	2·977	2·868	11·612	11·132
III	2·621	2·615	11·708	11·512
IV	3·026	3·001	11·816	11·688
1954—				
I	3·231	3·166	12·212	12·212
II	3·091	3·146	12·068	12·240
III	2·738	2·840	12·276	12·500
IV	3·258	3·290	12·720	12·812
1955—				
I	3·368	3·311	12·732	12·772
II	3·425	3·377	13·380	13·140
III	3·055	3·041	13·648	13·396
IV	3·615	3·492	14·128	13·600
1956—				
I	3·870	3·602	14·672	13·892
II	3·664	3·538	14·324	13·768
III	3·045	3·045	13·608	13·412
IV	3·574	3·386	13·976	13·188
1957—				
I	3·623	3·492	13·708	13·468
II	3·460	3·427	13·484	13·328
III	3·149	3·153	14·076	13·888
IV	3·696	3·554	14·444	13·844
1958—				
I	3·640	3·525	13·768	13·597

* Fourteen weeks.

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rate of seasonally adjusted consumption. On deduction of this from the estimated crude quarterly stock change, there results the non-seasonal stock change, and this added to seasonally adjusted consumption gives seasonally adjusted purchases. The figures used are given in Table 12. The curves of exports and net exports (exports—imports) are derived from actual weights imported and exported of steel of all kinds as listed in the Trade Accounts and summarized in Tables 89 and 99 of *Iron and Steel Annual Statistics* 1957.

Fig. 2.—Steel consumption, in ingot equivalent, is reckoned as crude steel production, plus reusable steel and imports, less exports, less (from 1937) producers' stock increase. From 1946, merchants' and consumers' stock increases are deducted also, with some uncertainty in the earlier years. The pre-war figures (given partly in Table 2 of *Iron and Steel Annual Statistics* and for the earliest years by Shone, 1947) treat wire as finished steel, and those now published for post-war years treat wire as a manufacture using a finished steel product (wire rods). The later figures have accordingly been adjusted to conform as nearly as possible to the same definition of steel as the pre-war data.

For industrial production, use has been made of the pre-war estimates given by Ridley (1955).

Fig. 3.—Figures are taken from *National Income and Expenditure* 1957, the 1954–62 increases being in proportion to those shown in Table 4A.

Fig. 4—Seasonal adjustments, where used, are those of the London and Cambridge Economic Service.

Figs. 5 *and* 6.—For U.S. industrial production, the Federal Reserve Board's index is used for 1919–56. The index covers mining and manufacturing but not building. For 1899–1918 an index was compounded of the mining and manufacturing indices issued by the National Bureau

TABLE 13

Industrial Production Indices and Steel Supplies, United States

(For P_t 1947–49 = 100, and P_n is in the same units. S and S_{calc} are in million tons of 2,240 lb., ingot equivalent)

Year	P_t	P_n	S	S_{calc}	*Year*	P_t	P_n	S	S_{calc}
1899	15·8	15·8	10·0	9·8	1928	53	56·4	49·2	46·7
1900	16·5	17·0	9·3	10·7	1929	59	58·1	53·7	54·2
1901	18·2	18·2	12·9	13·0	1930	49	60·1	38·9	39·1
1902	19·9	19·9	15·1	15·1	1931	40	61·8	25·2	24·7
1903	21·0	21·3	14·7	16·4	1932	31	63·5	13·5	12·0
1904	20·2	22·6	12·8	14·9	1933	37	66·1	22·6	19·0
1905	23·7	23·9	19·1	19·5	1934	40	68·4	24·9	21·9
1906	25·2	25·1	22·1	21·3	1935	47	71·6	33·1	29·9
1907	26·0	26·3	22·1	22·0	1936	56	74·3	46·5	40·9
1908	22·4	27·5	13·1	16·7	1937	61	77·1	47·4	46·2
1909	26·2	28·8	22·7	21·5	1938	48	79·8	26·3	26·4
1910	27·9	30·1	24·6	23·5	1939	58	82·8	44·0	38·6
1911	26·9	31·1	21·4	21·7	1940	67	85·3	50·7	49·7
1912	30·3	32·4	28·2	26·0	1941	87	..	66·3	..
1913	32·2	34·0	28·5	28·0	1942	106	..	68·3	..
1914	30·2	35·5	21·9	24·6	1943	127	..	70·8	..
1915	34·6	36·9	28·4	30·2	1944	125	..	72·6	..
1916	40·8	38·1	36·6	38·4	1945	107	..	64·8	..
1917	41·2	39·8	38·6	38·2	1946	90	103·0	53·4	74·9
1918	41·0	41·2	38·4	37·3	1947	100	106·7	65·3	82·3
1919	39	42·5	29·5	34·0	1948	104	109·9	73·5	85·8
1920	41	43·8	36·5	36·1	1949	97	114·0	63·7	73·3
1921	31	44·9	17·0	21·6	1950	112	119·1	83·9	90·9
1922	39	46·0	33·1	32·3	1951	120	125·0	92·4	98·1
1923	47	47·3	42·4	42·9	1952	124	129·7	79·4*	100·5
1924	44	49·0	35·8	37·8	1953	134	134·3	97·6	111·3
1925	49	50·5	43·4	44·1	1954	125	140·9	78·8	94·1
1926	51	52·5	46·2	45·9	1955	139	146·9	104·5	109·6
1927	51	54·4	43·0	44·9	1956	143	153·1	102·9	110·7

* Reduced by labour dispute.

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of Economic Research, so weighted that the composite index approximated closely over 1919–39 with the F.R.B. index.

Steel supplies are taken as production of ingots and castings, plus ingot equivalent of imports less ingot equivalent of exports. Changes in consumers' and merchants' stocks are nowhere allowed for.

The straight line in Fig. 6 is fitted by least squares (to determine A and α in equation (3), Appendix A) by means of the data for certain years only— 1899, 1901–02, 1905–06–07, 1915–16, 1922–23–24, 1928–29, 1937–38–39–40. These years were chosen because in estimating A and α it seemed unreasonable to pay much attention to years of considerable cyclical slack. Table 13 shows the values of P_t, the index of industrial production, P_n, the corresponding notional full-employment index, S the actual steel supplies and S_{calc} the steel supplies calculated from equation (3) Appendix A, with $c = 1{\cdot}4$ and A, P_0, as in equation (2).

Table 1.—The 1948–57 figures for the United Kingdom are based on official publications. Gross domestic product is at factor cost in 1948 prices. For the European Coal and Steel Community, the figures published by the High Authority have been supplemented by communications from the Secretariat, which are gratefully acknowledged. For 1955–65, the division of the annual increase in industrial production (4·9 per cent) between increase of working population (0·9 per cent.) and increased productivity (4·0 per cent.) is estimated by the authors.

Table 2.—For sources of data, see notes on Fig. 2.

Table 3.—The estimates of Ridley (1955) have again been used for pre-war industrial production The assistance of the Ministry of Labour and National Service is acknowledged in the estimation of the early changes in industrial working population.

Table 4.—The 1954 figures are mostly from *National Income and Expenditure*, 1957 and Balance of Payments White Papers. The allocation of taxes less subsidies on the different forms of expenditure (deducted to reduce the figures to factor cost) is at some points arbitrary. The 1954 division of Gross Domestic Product between "index of production" industries and others, based on Table 9 of *National Income and Expenditure*, involves an arbitrary division of stock appreciation and residual error.

Tables 5 *and* 7.—(The numbering of the lines is the same in each table.) The allocation of steel between sectors in 1954 is uncertain, the only definitely recorded figure being that for coal mining. The National Coal Board maintains records of steel consumed, which are used throughout in Table 7. Other sectors' 1954 consumption has had to be guessed from various sources, in particular the following:

(a) *Steel-makers' deliveries to the various consuming industries in* 1954 *and later.*—Purchasers are distinguished in 26 categories related to the Standard Industrial Classification. The purchasers' industrial classification may not correspond, however, to the use to which the steel is actually put. Furthermore, over a quarter of steel makers' home deliveries go to merchants or to makers of intermediate products such as wire, nuts and bolts, drop forgings, the final use being unrecorded. The destination of imported steel is also unknown.

(b) *Allocations made for various final products in* 1949 *and* 1952 *under the statutory schemes of steel control.*

(c) *The Censuses of Production*, 1951 *and* 1954.

(d) *The consumers' stock census.*—Returns made in respect of each calender quarter, showing consumption and stocks of steel, rendered by firms and public boards accounting for about five-sixths of national steel consumption.

(e) *Technological estimates of the requirements of steel per unit article.*

These sources are liable to show inexplicable differences of 10 per cent. or more. So far, the various revisions of weighting as the work has developed have not resulted in any significant revision of the total of calculated consumption in 1962, and have slightly improved the correspondence of calculated and actual consumption over 1950–56 in Table 7. As this correspondence, apart from what is apparently some timing discrepancy in 1955–56, is so very close, it should perhaps be made clear that of the adjustment of weighting none has been made on the grounds that it would improve correspondence. The close correspondence must be largely mere coincidence, for there are good reasons for believing that the actual steel consumptions for separate sectors vary otherwise than the calculated sector consumptions. The note at the foot of the table must be stressed.

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The 1950–57 variations in the different sectors are estimated as shown below.

Line 1. Estimate based on Admiralty's record of work done and steel received.

Line 2. Steel consumption recorded by the National Coal Board.

Line 3. Mainly steel consumption recorded by railways and by rolling-stock manufacturers, less an allowance for exports by the latter.

Lines 4, 5 and 7(*b*) Quarterly estimates of National Expenditure as published by the Central Statistical Office in the *Monthly Digest of Statistics.*

Lines 6 and 7(*c*) *National Income and Expenditure* 1957.

Line 8. Official production index, Orders III, IV, X–XVI, value-weighted as published in the *Monthly Digest of Statistics.*

Lines 7(*a*), 7(*d*), 7(*f*), and 7(*g*) Economic Survey 1957, Table 24 and corresponding tables in Economic Surveys for 1956 and 1955. An index has been constructed over the years 1950–56 by linking the year-to-year changes shown in the latest published annual survey in which the figures for any pair of successive years are shown side by side.

Table 8.—The 1950–54 changes in expenditure are based on officially published figures at 1948 market prices. The exports are those in line 7(*e*) of Table 7.

Table 9.—The items are those appearing on the right-hand side of the account obtained by amalgamating Tables 4A and 4B (imports and exports being cancelled from both sides). The total is national income and capital consumption, in factor costs. 1954 costs are used for 1954 and 1962, and current costs for 1955–56–57. To give a run of comparable figures the table conforms throughout to the revisions in "Preliminary Estimates of National Income and Expenditure 1952 to 1957" March 1958. The 1954 and 1962 percentages are thus slightly at variance with tables 4A and 4B which it was impracticable to recast.

Discussion on the Paper by Sir Robert Shone and Mr. Fisher

Mr. C. T. Saunders (proposing the vote of thanks): My first duty is to express to Sir Robert Shone and Mr. Fisher the very sincere gratitude of the Society for so frank and fully informed a description of some of the major considerations underlying the steel expansion programme. Our gratitude should extend beyond the present paper to the full account of the economic background and projections in the official Iron and Steel Board document on the expansion plan—the Special Report of 1957. It is quite common, nowadays, for economists and statisticians to play some part in arriving at important decisions of economic policy in private business, in government and in the rather important half-world between. What is rare is that they should be able, and willing, to describe so adequately and so concretely the intellectual processes and economic considerations which contribute to the discussions. Philosophers may become kings—but, too often, they thereupon cease to care about philosophizing.

From the point of view of economic statisticians, with some confidence, I hope, in our own tools, we must congratulate Sir Robert, Mr. Fisher and their colleagues on the full and often highly ingenious use which they have made of the resources of economic statistics both in drawing up the projections and in following current trends.

Naturally, one realizes that Governments, Public Boards and private businesses do not take important decisions solely on the basis of economists' projections. There are other crystal balls to gaze into, at least as reliable as the economists'. But this is, I think, the first occasion on which a major decision of long-term economic policy has been publicly justified, and exposed to criticism, in terms of a detailed and coherent judgment—a judgment not a forecast—about the future pattern of the economy.

The formation of a set of projections of this sort for an industry depends on there being available a very full body of statistics about the use and disposition of the industry's products. The British steel industry is—or was until very recently—unique among the steel industries in Western Europe and North America in that detailed statistics are available

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of deliveries of steel to various consuming industries, and of consumers' and merchants' stocks, from which adequate—although by no means perfect—estimates of steel consumption by industry, by qualities of steel, may be derived. This is largely due to the work of the British Iron and Steel Federation over a long period, and also to the willingness of steel producers and users to provide the necessary information. The absence of such statistics has, I think, made it impracticable for development plans, comparably well-founded on detailed knowledge of past experience, to be drawn up in Western Europe.

Within certain limits, it is hardly possible to argue about the general rate of economic expansion. It is completely impossible to *predict* in any rational sense whether production will rise by 3, or 4, or 5 per cent. There is an upper limit which is, rightly I think, set not by consideration of physical productivity, but by balance of payments considerations—that is by the extent to which exports might expand at something like the current terms of trade. (This might be looked at in another way—probably producing the same kind of answer—by arriving at an *optimum* rate of real expansion above which higher rates of expansion could be achieved only by a fall in export prices so great as to reduce the rate of increase of real income.) But, within these limits, an initial act of judgment was needed to set the framework for the development plan. The problem to which ratiocination can be applied is the future pattern of expenditure and production.

From the point of view of the *pattern* of expansion, the striking feature of the projections is that they depart in important respects from past experience. This is emphasized clearly enough by the authors. As shown in Table 1, a lower rate of economic progress in the future than in the past is expected to result in a higher rate of increase in steel consumption. Extrapolation from ratios of steel usage to the usual aggregates—e.g. gross national product—would not have produced this result, as Section II and Fig. 2 are devoted to showing, and would, if the development plan is justified by events, result in a significant shortage of steel capacity. It looks very much as though simple extrapolation by reference to aggregate indicators is the basis for the projections of the Coal and Steel Community in Table 1.

It is important to realize that the "steel-intensive" economy is based on a particular pattern of general economic development; the controversial issue is the ratio of investment to consumption, which is assumed to rise fairly steeply. Table 9, relating to the disposition of national income (to be pedantic, it should be gross national product) implies a rise in total investment, at home and abroad (or in total saving) from 16·7 per cent. of gross national product in 1954 to 19·1 per cent. in 1962. This ratio has hitherto been a slow moving one and the authors may well be right (in paras. 64 and 65) in suggesting that this diversion of national resources may not occur by itself.

This raises two questions: the first is how far the authors would regard this characteristic of the changing economic structure as a "best guess" of what will in fact happen, how far as a desirable—or perhaps they would say a necessary—objective of policy.

The second question is how far the steel programme would itself be affected if this shift in pattern did not in fact occur. It is reasonable to expect that almost *any* imaginable increment of total expenditure will be steel-biased, since increases in exports, or in consumption, as well as in investment, will tend to fall heavily in the field of metal goods. But is marginal investment *very much* more steel-biased than marginal consumption?

There is certainly some evidence in Fig. 2 that the lift in the ratio of steel usage to total production has begun to show itself since 1954. This is a reflection of the lift in investment, and in domestic car sales, in 1955 and early 1956 which, as Fig. 4 shows, were to some extent compensated for, with some help from the management, by the temporary collapse in consumers' expenditure on household durables.

There is a warning sign in Fig. 3—the lift in the ratio of imports to total final output which also occurred in 1955 and 1956. This was itself due in part to the marginal imports of steel (and coal) but only in part, and the ratio has certainly not fallen in 1957.

One of the most familiar and most difficult problems for those executing, as distinct from those creating, a plan based on projections is to know when to change one's mind, which means mainly how to distinguish the short-term aberration from the change in long-term trend. Consideration of the plan began in 1955, and the base year used was

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1954. Since then, history has not always seemed kind. Yet, in the light of the hindsight so far possible, the Board seems to have been justified by events. So far, as Fig. 4 shows, the principal variables in their short-term wanderings have shown an admirable disposition to return, like well-trained, although sometimes playful, homing pigeons, to the lines of flight prescribed for them. May the successors of these charts look as good in 1960. But my feeling is that some more aberrations will have occurred before then. We can only wish good judgment, and good luck, to Sir Robert and his colleagues in deciding when to revise and when to stand firm.

Sir CHARLES GOODEVE: I have great pleasure in seconding the vote of thanks and also in congratulating the Society, Sir Robert and Mr. Fisher, for putting this extremely important matter before us. The steel industry certainly looks upon itself as fortunate in having at its disposal both the statistical information referred to by the proposer and also the techniques which have very largely been developed by Sir Robert himself, helped by his colleagues.

I had the pleasure of reading and hearing his previous paper given to the Society (this *Journal*, 1947). I still remember one important point of that paper—cyclical or counter-cyclical relation between the rise of capital scrap and steel production. Sir Robert showed that whenever steel production goes up the supply of capital scrap goes down and *vice versa*, with a displacement of about one year. In ten years from now I shall remember quite a number of things about this present paper.

I am neither a statistician nor an economist, and I particularly appreciate the paper because it is put in physical terms and not in the metaphysical terms of many economists. There is no reference to "supply of capital" or other mysterious terms used inside the City of London.

Sir Robert uses the term "elasticity" which slightly worries me. A physicist or an engineer has ideas about that term and economists normally use it in much the same way; the rise of a force (prices) resisting a displacement (demand). But here it is used merely for a correlation coefficient between two extensive properties expressed as logarithms. The power of entropy manifests itself even in language!

The prediction of Sir Robert Shone and Mr. Fisher depends very much on an increase of productivity of 3 per cent. per annum. I do not think sufficient attempt has been made to justify that increase except by historical extrapolation—not a very good method as any statistician will agree. I think some support can be obtained from a comparison with the United States. Since Rostas's famous work, we have raised the productivity of labour in this country so that it is now nearly one-half that of the United States compared with one-third before the war. This means that by applying to-day's American standards to our industry we could increase productivity 100 per cent. In the light of this the increase of 3 per cent. per annum is surely the minimum that we should plan for.

Sir Robert has said that somebody to-day might say that his prediction made eleven years ago was wrong in that it was an under-estimate and to-day he might be accused of leaning the other way for safety. In planning for the future one obviously must not just think of one's error in terms of thousands of tons but in terms of the relative cost to the community of a shortage of steel compared with the costs of having surplus capacity. I think fears of the latter are often very much exaggerated. In 1954 steel production in the United States fell by 30 per cent. yet the profits of the companies fell very much less. To-day production is down by 50 per cent. and yet the Americans are adapting themselves so well that their losses are small. These are enormous short-falls in prediction compared with the hoped for accuracy in this paper. In other words the steel industry need not worry very much if it does turn out to have some surplus capacity. The penalties of over-estimating by a million tons per annum are much less than those of under-estimating.

I think that the last part of the paper must have been read with even more interest than the earlier part. Sir Robert brought out a fact which has disturbed many of us, that in a democratic society we are going to have a great problem in encouraging or inducing people to save or invest properly; they are much more interested in taking out of the economy all

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that they can get. In the face of the many other countries who are investing more rapidly than we are, we must educate people to a higher and broader standard than we are achieving to-day, particularly in economics. We hear a lot about freedom but what we need is *freedom from ignorance*. Sir Robert and Mr. Fisher have done a great deal in that particular direction; we can only wish their paper or its ideas could go to a wider public.

The vote of thanks was put to the meeting and carried unanimously.

Mr. E. F. SCHUMACHER: I have one small and important point to make and one large and relatively unimportant one. The first is that, on p. 280, the paper makes some predictions about the coal industry having no coal for export, and I personally would be very happy if this could disappear from the published version because the coal industry is in fact in the market to export coal on a large scale.

My second point is of too much estimating. Reading the paper with great admiration I could not fully suppress the thought "are these journeys all really necessary?" We have heard Mr. Saunders say, and I entirely agree with him, that there is no objective basis for settling on 2 per cent., 3 per cent., 4 per cent. or 5 per cent. as the general rate of expansion; this is an act of judgment—some people judge better than others but it is an act of judgment. The choice between 3 per cent. and 4 per cent. seems to me to overshadow the effect of any of the refinements later introduced into the paper. What difference does it really make whether we use the factor of 0·8 or the factor of 1·0 as the relationship between industrial output and steel consumption, if we really do not know whether industrial output is going to rise by 3 per cent., 4 per cent., 5 per cent. or 2 per cent.?

This is brought out almost embarrassingly clearly in Table 7. I would plead with all sincerity with the authors to have another look at Table 7 because it seems to me to be simply too good to be true. If an estimate applied to a previous period comes out too exactly, I suspect that I am treating as an independent estimate some factor which I had in fact arrived at as a residual. To work back with the authors' method from 1954 to four preceding years and to come out with a total calculated steel consumption every time within a fraction of 1 per cent. of actual indicates, I suggest, that something must have gone wrong. I am not a probability statistician, but I consider the probability against this to be a matter of quite colossal magnitude. I see, for instance, that the steel consumption of coal mining and of the railways has been treated separately. That was a lucky hit because if these two lines had been treated the same way as all the other lines the error would have been a matter of several hundred thousand tons. Something must have gone wrong in the earlier years, because the 1955 and 1956 figures show a decent sort of error.

Is it really worth while going to all these lengths of estimating? Do we get any further or are we clouding our own judgment? If I have to make a broad judgment "is this going to be 3 per cent. or 4 per cent." I inform myself as much as possible about the past, and then make the judgment; then perhaps times change and I know exactly 4½ per cent. was a guess and it might be safer now to reduce this to 3½ per cent. I make one guess and one reduction, but if I have all these figures they give me the idea that I know the future: do I then, in fact, really reduce every one of them as a separate estimate? Is this not a spurious kind of thing? I hate to say this in this company because it might considerably reduce the demand for highly qualified statisticians, but I think it is a most interesting matter which merits very serious thought. Certainly in the fuel field people take various projections into the future as absolute gospel truth—totally forgetting they are pure crystal gazing—merely because they are supported by so much detail.

Dr. T. BARNA: I should like to join previous speakers in stressing that this is an important occasion. We have before us forecasts for a major industry which is one of the heaviest users of capital assets. Although all forecasts are subject to error, the way the reasoning underlying the estimates is set out should help the steel industry to reach better decisions on its investment policy.

There is one important point which the authors have made: in forecasting the demand

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for steel the growth of the total economy and the possibility of structural change are equally important. In forecasting the demand for the products of most other industries structural change is likely to be more important that the growth of the economy. I am not sure whether this makes the steel industry easier or more difficult for the statisticians but it certainly makes it more fascinating.

On the substance of the forecasts, I agree with the general underlying assumptions that the rate of progress in the economy in the post-war years has been greater than before the war and also that we are moving towards a more steel-intensive economy. The case for more steel is brought out even better if we look at the growth of industrial capacity rather than at the growth of actual industrial output. It is remarkable that in the last 2½ years, while total industrial output has hardly changed, investment and the consequent increase in capacity continued at a high rate. This is perhaps not fully realized in the steel industry and one day when industrial output catches up with the growth of capacity we are almost certain to run into another steel shortage. The assumption of the forecasts before us is of course that steel output is dependant on industrial production; I am sure that in some years in the recent past it was the other way round and with a higher output of steel we could have had a higher industrial output.

On the effects of structural changes in the economy, the authors give only one set of estimates but it would be desirable to examine the effects on demand for steel of alternative "models" of the economy. I should mention that, for the United States, research into inter-industry relations by the statisticians of the Bureau of Labour has shown that both a high-investment and a high-consumption model of the economy leads to approximately the same demand for steel. I wonder whether such conclusions would hold for this country also.

I should also like to suggest that the analysis based on time series could be reinforced by cross-section data comparing different countries. I am sure that steel consumption in this country would appear relatively low compared with the United States or Western Germany. We have not gone far enough to replace our declining industries with others which are mostly steel consuming industries. The case for forecasting a tendency towards a more steel-intensive economy is indeed strong.

Finally, I should like to point out that the economic costs of an over- or under-estimate of future steel requirements are not identical. The cost of over-estimation is the carrying cost of excess capacity—say 10 per cent. per annum on £80 per ingot ton, i.e. £8. The cost of under-estimation is the net value of finished engineering output lost which is of the order of £100 per annum per ingot ton of steel. It is true that shortages can be mitigated by imports or by drawing on stocks but experience has shown that at the time of a domestic shortage imports may be also difficult to obtain and that stocks tend to increase rather than decrease when demand for steel rises.

Mr. E. T. SARA: When Sir Robert Shone presented his paper eleven years ago Professor Cairncross and I were the two to whom he has referred as having said he was too cautious. Dr. Barna is inclined to take the same view, but I doubt whether it is valid to-day.

It is, of course, necessary to calculate total demand, but the criterion as to whether a unit of capacity is installed or not should be whether it is likely to be profitable. This is certainly the right point of view for the firm concerned. This supports the point made by Sir Charles Goodeve but I would remind him that if one looks at the latest figures in America it will be found that over the past five or six years they have had a very large proportion of unused capacity.

In Appendix I of this paper I notice that for America, the post-war figures are all below the curve, whereas for this country they tend to be above it. It might well be that the Americans are moving into a period when steel consumption is not increasing as fast as it was in relation to total production, and also they are able to create assets much more freely than we can in this country and are therefore more likely than we are to have a surplus of capacity.

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Mr. A. T. ORAM: I would like to refer to Fig. 2 and the equation for the relationship between steel consumption and the index of industrial production. It is explained in the text (para. 20) that "The form of equation (1) was suggested by the following hypotheses. There is a considerable section of industry (including for example, textiles) which uses, at least directly, practically no steel, and this section has shown very little growth over the period. It is represented in the equation by the 30 subtracted from P . . . ". I should have thought that this was the wrong model and that the figure of — 30 represented (rightly or wrongly) the production index when steel was first introduced. In the early days new uses for steel were being developed at a great pace, giving rise to a steep curve at that stage, after which it flattens out. It is unfortunate that just at the time when the curve has flattened out the equation is set up and that a form of equation has been chosen which suggests that the line will be straight (on logarithmic paper) for the future. I would suggest that we have no sound evidence that the elasticity will be asymptotic to 0·795 as is indicated in the equation; it would seem to be more reasonable to assume that it would come down a little as the index of industrial production increases. The index of industrial production is being considerably influenced by new industries, such as plastics, and there may be other new industries yet to come which will not directly consume great quantities of steel. Through their effect upon the production index such developments must tend to reduce the elasticity, quite apart from and in addition to any question of the substitution of new materials in place of steel. If similar graphs were produced for other commodities the results might bring out more clearly the influence of new uses, but the interpretation is bound to be complicated by the fact that the scale showing the index of industrial production is to some extent but not entirely a time scale. Nevertheless I would suggest that the question of new uses should have been given some weight in the model behind the equation.

Mr. P. MOORE: One thing in the paper rather disturbs me—it seems that the judgment is not explicit with regard to the assumption of increased steel-intensiveness. We see the actual figures for different steel-using sections and from this are led to the logic of the increase in steel intensiveness. The only thing which is not considered and might well be considered is the question of substitution against steel. New materials are coming along which might to a certain extent invalidate the assumption that the economy is going to become more steel intensive. I feel some mention of this aspect could have been made. Even when one is looking at the motor industry it must be borne in mind what may happen in terms of plastic bodies replacing pressed steel. The possibility of substitution by new materials should be explicitly recognized in forecasting; even if only to state that it is considered to be of relatively small importance. It might well have considerable importance in this field. If in fact such substitution is, or is likely to be, an important factor, then it appears to me that it could be calculated and taken into account in arriving at the final forecast.

Professor F. W. PAISH: I should like to ask Sir Robert to do a rather more detailed forecast for the future. If you look in one crystal ball you have nothing with which to check the answer; if you look in two crystal balls and they agree there is the possibility that they are both right; if they disagree they cannot both be right, and I think when forecasting your national income in the future you must look at 50 or 100 crystal balls and make them all consistent with each other. A guess may seem reasonable by itself, but when a lot of other guesses are made which also seem reasonable they do not fit; either the other guesses are wrong or the first one is wrong. By the time you have tried them all you are probably not right with any of them but they are at least consistent.

On the question raised by Dr. Barna about forecasting, it is not so important to foresee the thing which is likely to be right, as the thing most likely to do real harm if it is wrong. Professor Devons pointed out in his book *Planning in Practice* that the statistician was often required to provide figures about which there was a great deal of uncertainty and on which his superiors would take important decisions. If he gives margins of error they

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will be ignored and if he gives probable limits his superiors will just add them together and divide by two, so what he must do is to come to a decision about what decisions his superiors ought to take and then put up whatever figure he thinks most likely to lead them to the decision which he thinks right, and that is what I imagine Sir Robert has done.

Sir Robert Shone and Mr. Fisher subsequently replied in writing as follows:

Mr. Saunders asks how far is the projection a "best guess" of what will happen and how far is it a desirable objective of policy. It is certainly rather more than a guess about the future. The projection does involve taking a view about what is necessary for an expanding and dynamic British economy. In adopting a particular rate of expansion we worked on the principle that (to quote from the Iron and Steel Board's Second Development Report) "Steel must be provided to support as high a rate of increase in national output and income as seems reasonably likely to be possible". There is certainly an element of choice in the Steel Plan: we knowingly choose a rate of growth which will come about only if such growth is wanted by the country and if many individuals and industries contribute to the pattern of development that it implies.

Mr. Saunders also raises the question whether the estimated steel demand would be significantly different if we had a high-consumption rather than a high-investment economy. The same point is raised by Dr. Barna, who quotes American experience which tends to show that steel requirement might be little affected. This we think would be truer of the United States than of Britain, where marginal consumption expenditure may be more heavily geared to metal durable consumer goods. In Britain the steel content of marginal investment seems to be substantially higher than that of marginal consumption. The

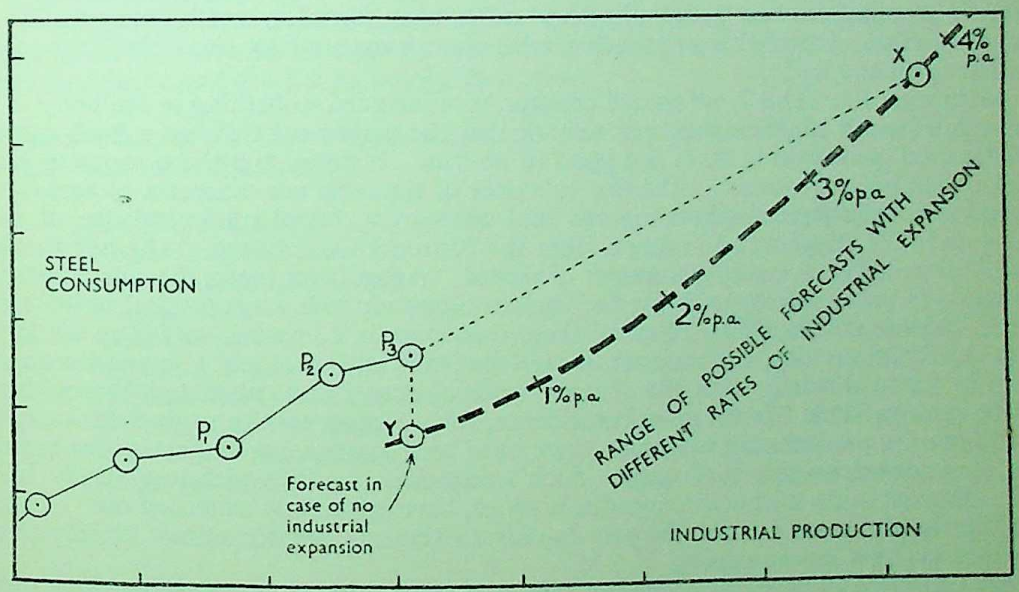

FIG. 7.—Effect of assuming different rates of industrial expansion.

relevant choice, however, does not seem to be between the division of a given future total national product between investment and consumption. The choice is between economic expansion, with a high investment proportion, and on the other hand low investment with ultimate low consumption too, because the economy fails to grow. We cannot here meet Dr. Barna's suggestion of presenting a number of alternative models; but alternatives have been worked out and their broad interrelationship is sketched in Fig. 7. This constitutes, not to scale, an elaboration of the top right-hand corner of

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Fig. 2. The annual points . . . P_1, P_2, P_3 represent recent experience. The projection X for a given future year, such as 1962, is the outcome of a moderately high rate of expansion. The projection Y represents a theoretically possible very pessimistic forecast for the same future year, in which industrial production is supposed to have expanded not at all since the base-year. This implies that there will be practically no net investment; the switch from net investment to consumption, with no global economic expansion, implies a fall in steel consumption. Thus Y lies directly below P_3. Between Y and X, or beyond, there is a range of possible forecasts, i.e. outcomes of smooth-trend evolution of the economy up to the target year. These may be thought of as composing a curve like the broken line in the diagram. This curve represents not a temporal sequence but, we repeat, a range of possible forecasts for the one target year. (Rough estimates suggest a curvature of XY in the sense shown.) The steepness of the XY curve (exceeding that of any growth-line such as P_3X) brings out the fact that changes in our estimate of the growth of the economy are likely to cause more than proportionate changes in our estimates of steel requirements.

This fact constitutes one of the answers to Mr. Schumacher's doubts on the usefulness of detailed thought on the magnitude and pattern of future growth. Mr. Schumacher asks what difference it makes if an elasticity of 0·8 or 1 is used in relating steel consumption to industrial production or if a growth of 2 per cent. or 5 per cent. is used in forecasting the growth of industrial production. The difference would be between providing at the one extreme for an increase of 2 million tons in steel requirement between 1954 and 1962, and providing 8 million tons of increase, a difference in money expenditure of possibly some £600 million. It may be that great errors will be made in looking forward to some particular pattern of development of the British economy, but some forward view has to be taken in planning for new plate mills, sheet mills, blast furnaces, and it seems well to set out these views as explicitly as possible, even though events may show they have to be revised again and again.

With regard to Table 7, we should perhaps have included something in the body of the paper to forestall Mr. Schumacher's remark that the agreement between actual and calculated steel consumption looks too good to be true. It does; but there seems to be no explanation but coincidence. The first two lines of the table are estimates of actual consumption in merchant shipbuilding etc. and coalmining based on knowledge of work going on in the shipyards and reports from the National Coal Board. The allowance in line 3 for railway track is also a matter of record. Apart from these, the calculated consumption is got simply by applying the various economic indicators to steel weights estimated beforehand for 1954. None of these indicators is compiled, so far as we know, with any significant degree of reliance on steel statistics, such as could give spurious agreement in the final total. That the *general trends* of actual and calculated figures should be the same in Table 7 is the main coincidence, for we know that in some fields—exports of engineering products, for example—there have been appreciable tendencies for tonnage and constant-price value to diverge. Such tendencies, together with any steady biases that may exist in the output or expenditure series, have apparently cancelled out. That in addition to the general trend the *year-to-year* movements are sometimes closely reproduced is perhaps less surprising.

Sir Charles Goodeve has suggested that it is more serious to under-estimate future demand for steel than to over-estimate. Dr. Barna again has given an arithmetical illustration of the same point. It is only fair to point out that in contemplating an extra million tons of steel capacity in the region in which it is expected that demand will normally be fully satisfied, one would be deliberately planning for extra steel capacity on the assumption that there will be periods when the full capacity of the industry will not be required. It is by no means easy to calculate the net gain or loss in providing for peaks. It must be borne in mind, however, against Dr. Barna's simplified calculation that frequently peaks do not occur throughout the world at the same time. For example, there is now a surplus of sheet in America and a deficiency in the United Kingdom; there is also an inherent flexibility in steel capacity which enables output at a cost to be pushed appreciably above

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the optimum level. Further, in the particular estimates used for the current Steel Plan the starting point has been a condition of full employment and on top of that a margin of 3 per cent. additional demand has been taken into account.

Both Mr. Oram and Mr. Moore observe that more attention might have been given to the question of possible substitution between steel and other materials. Some provision has been made for this substitution, as can be seen from Table 5; and Table 7 shows, for what it is worth, that over a seven-year period the net substitution was unimportant. We do, however, fully recognize that this is a matter which has been dealt with altogether too lightly in the paper and that it is worthy of a complete study in itself. It requires constant attention. For example the experience of 1957–58 has thrown doubt on the expectation (see paragraph 47) that the increases in use of sheet and tinplate will continue to exceed the increases of activity in the corresponding consuming sectors. Considerable quantities of these types of steel go into containers, a form of use where new methods and materials seem to be encroaching. At the same time, tinplate containers are finding entirely fresh uses, which may turn the scale the other way.

Another important practical point not dealt with in the present paper is the need for modernization in the steel industry. This need makes it important that the current Development Plan should retain its momentum, even though the course of demand itself is inevitably uncertain and even if we run into a period of recession.

The authors are fully aware of the uncertainties of the future both on the particular point of substitution as well as the great problems posed in a democratic society of securing the restraint of consumption on which an investment-intensive economy can be based. It is because of this concern that they have felt justified in the latter part of the paper in speculating on some of the conditions which might be helpful in securing a satisfactory rate of economic growth.

Finally, we should like to thank all the speakers, including Mr. Sara and Professor Paish on whose remarks we have not specifically commented, for their kind observations about the paper and their most helpful comments.

As a result of the ballot taken during the meeting the candidates named below were elected Fellows of the Society:

Gordon Addison
Roger Harold Ashby
John Makepeace Bennett
Tom Chellew
Alun Davies
Frederick Byron Davies
Norman Henry William Davis
Thomas Kevin Gribbin
Jack Ernest Hartshorn
Ralph Henstock
Duncan Standon Ironmonger
William Lloyd James
Dennis Kernan
John Rigby Knox
Christine Margaret Loveland
William Ian Robertson Martin
Frederick William Newell
George Harvey Robinson
Brian William Sayer
Kenneth Anderson Smart

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Some Problems in the Measurement of Price Changes with Special Reference to the Cost of Living

A Discussion opened by Dr. S. J. Prais

[Held before the Royal Statistical Society, April 16th, 1958, the President, Sir Harry Campion, C.B., C.B.E., in the Chair]

1. *Introduction*

In the many years that have elapsed since Jevons and Sauerbeck presented papers to this Society on the measurement of price levels, an extensive literature has accumulated on this subject. It has grown especially rapidly in recent years together with the widespread growth in the provision of statistical information, which governments—in all parts of the world—to-day find necessary for carrying out their economic policies. The rapid rise in interest in the subject is evidenced in the recent bibliography on index numbers of Allen and Buckland (1956), containing some six hundred entries: of these, a half relate to publications in the last decade.

While there is still scope for fundamental research (as, for example, in the development of constant utility index numbers), practitioners have come to substantial agreement on what a cost-of-living index number should to-day measure in principle, and on how measurement should be made in the first instance. Within the accepted framework there is, however, still room for the improvement of practice, and for the delineation of a best practice. Progress here tends to be slow, though a start towards a systematic treatment of the major practical issues has been made in the O.E.E.C. publication by Stone (1956) and, on a narrower front, in the work of Hofsten (1952).

In introducing a discussion this evening on problems in measuring changes in the cost of living, I have chosen to concentrate on three topics which seem to me to deserve fresh examination. But, I rapidly add, I have no desire to restrict the discussion of other participants to these alone; indeed, I would be happy to hear other views as to which problems in this field deserve priority to-day. The three topics considered here are, first, the possible inaccuracy in the weights of a cost-of-living index number, which may result from non-statistical biases in surveys of household expenditure; secondly, the conceptually correct treatment of housing costs for those families that own their houses; thirdly, some reflections on the treatment of durable goods and the service charges on hire-purchase instalments.

2. *Biases in Consumer Surveys*

In collecting information on household expenditures by sample surveys it is widely recognized that biases may arise because a certain proportion of households refuse to co-operate, and their pattern of expenditure may differ from those that do. Our understanding of these and associated problems owes much to the well-known work of the Research Techniques Division of the London School of Economics. But biases also occur among the apparently fully co-operating households, partly because recorded ex-

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penditures differ for various reasons from the true expenditures, and partly because expenditures in the period of the survey differ from those that would have been incurred if the survey had not taken place. We may therefore say that the total bias—that is, the difference between recorded and normal expenditure—consists of a "recording bias" and an "expenditure bias".

Information on the magnitude of these biases is scarce. In the nature of the matter, if the holding of an inquiry into consumers' expenditures by itself affects those expenditures, it may seem impossible to obtain any direct check on accuracy (this, I believe, is popularly known as another manifestation of the Heisenberg principle). Indirect methods have therefore to be adopted. Occasionally one may learn something by comparing total supplies of a commodity with survey results (as, for example, alcoholic drink and tobacco for which there are reliable excise statistics). Or, again, it may be possible to learn something by comparing two surveys that differ in some particular circumstance in which they were conducted.

2.1. *Length of Inquiry: Previous Results*

A change in the period of time for which expenditures are collected is an example of just such a circumstance which may well lead to some insight into the biasing factors at work, and their magnitude; an examination of the observed effects of such changes forms the subject matter of the remainder of this section.

Information from a number of previously published surveys is brought together in Table 1, which shows the percentage difference between alternative estimates of expenditures on various commodity groups. The surveys quoted employed different methods of inquiry, and the time period used varies from a week to a year.

The two pre-war surveys in the United Kingdom were based on account books in which households recorded their expenditure; for clothing, a comparison is here possible for expenditures over a month and over a year. Whatever the factors at work may have been, it is found that on balance they led to a decline in recorded expenditures for the longer period.

Comparisons for a more extensive range of non-food items is available from the paper read to this Society by Mrs. Cole and Mr. Utting in 1955. In this case the estimates were based, not on account books kept by the household, but on an interview which asked for expenditures in the past week, past month, past quarter, or past year. The results for the larger items, summarized in the present table, show the monthly estimates as higher than those based on the past week; but that for longer periods than a month (a quarter or a year) the estimates for the longer period again tend to be lower, and by very substantial amounts.*

For food items, certain information is available from an Indian survey. This also employed the interview method, and a comparison is possible here for weekly and monthly expenditures. The monthly estimates are here seen to be generally lower, again by substantial amounts; further evidence collected by Mahalanobis and Sen (1954, p. 10) leads them to think that it is the lower figures (the monthly estimates) that are nearer the truth.

* Only the larger items (those with a weekly estimate in excess of £10) are reproduced here in Table 1. Among the smaller items there are three exceptions where the annual estimates exceed the weekly estimates: house purchase, licences and stamp duty, and domestic service. But it seems quite possible that sampling fluctuations may here have masked the general tendency noted above.

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TABLE 1

Alternative Estimates of Expenditure on Various Items According to Length of Period of Inquiry

Country and Description	*Date*	*Reference*[a]	*Commodity*	*Units*	*Period of Inquiry*				*Difference*[b]
					Week	*Month*	*Quarter*	*Year*	%
U.K. working-class . .	1937–38	*A*	Clothing	Shillings/ household/ week	—	9·3	—	8·2	13
U.K. middle-class . .	1938–39	*A*	Clothing		—	22·9	—	15·5	39
U.K. Cambridgeshire . .	1953–54	*B*	Clothing	£/ household/ year	53·4	70·2	—	—	−27
			Minor durable goods		23·3	29·2	—	—	−22
			Major durable goods		89·7	—	—	41·8	73
			Coal and coke		20·8	—	13·3	—	44
			Electricity		10·2	—	7·8	—	27
			Life insurance		27·7	—	—	21·0	28
India	1952	*C*	*Quantities:*						
			Rice	Oz./ person/ day	16·21	15·14	—	—	7
			Pulses		1·56	1·19	—	—	27
			Sugar		0·23	0·16	—	—	36
			Salt		1·07	0·76	—	—	34
			Expenditures:						
			Food grains	Rupees/ person/ month	9·52	8·77	—	—	8
			Other foods		6·01	5·04	—	—	18
			All food		15·53	13·81	—	—	12

Notes: [a] Source references. *A*: See Prais & Houthakker (1955, pp. 29, 41). *B*: Cole & Utting (1956, p. 375), Table 5. *C*: Mahalanobis & Sen (1954, pp. 9–10).

[b] Excess of shorter over longer period estimate as percentage of their average.

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2.2. *Some Stricter Comparisons*

The comparisons quoted so far are suggestive of the seriousness of the recording bias, but are not conclusive. Many will perhaps not feel surprised at the large differences revealed by interview methods which inevitably rely on human memory; and the accuracy of the account book method is hardly impugned by evidence relating only to a single commodity group.

A more extensive analysis based on account books, and bearing on our subject, is now possible following a survey recently conducted in Israel: the results quoted here are preliminary and relate to about 3,000 households.* The organization of this inquiry had special features which, as will be seen, allow a number of interesting comparisons to be made.

The sample of households in this inquiry was divided at random into three approximately equal parts, termed "panels", differing only in the length of time during which the households recorded their expenditures on foodstuffs, while for non-food items all panels reported for the whole month. The first panel, A, was required to keep records of total expenditure, both food and non-food, for a calendar month. The other two panels recorded non-food expenditure for a whole month, but recorded food expenditure for half the month only. The difference between these latter two panels is only a matter of dates: one of them, B, carried out their food recording in the first half of the calendar month, and the other, C, in the second half of the calendar month.

For convenience of reference, Table 2 shows the layout of the inquiry. The symbol A_1, for example, represents expenditure on food items by households in panel A in the first fortnight; the subscript 2 indicates the recorded expenditure in the second fortnight. Dashed symbols indicate the corresponding quantities for non-food items.

TABLE 2

The Three Panels of the Inquiry: Layout and Notation

	Panel A		*Panel B*		*Panel C*	
	First Fortnight	*Second Fortnight*	*First Fortnight*	*Second Fortnight*	*First Fortnight*	*Second Fortnight*
Food items	A_1	A_2	B_1	—	—	C_2
Non-food items . . .	A'_1	A'_2	B'_1	B'_2	C'_1	C'_2

Note: The results quoted in subsequent tables relate to approximately 900 households from each panel.

First, as before, we may examine how recorded expenditure varied with the length of time for which records were kept. The closest comparison is between the monthly expenditure on foodstuffs recorded by panel A with the sum of the two fortnights recorded by panels B and C; in symbols, we examine the expected equality

$$A_1 + A_2 = B_1 + C_2 \quad . \quad . \quad . \quad . \quad . \quad (1)$$

It will be noted that, in contrast to certain of the preceding examples that have been quoted, the same calendar period of time is here compared on both sides of the equation; the comparison therefore relates strictly to the effect of the length of recording period, and

* This comprises about half the total sample. The final results of the survey will in due course be published by the Israel government's statistical bureau.

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eliminates the effects of any possible variation in expenditure between different calendar periods of time.

TABLE 3

Expenditure Recorded on Foodstuffs for Identical Calendar Period, According to Length of Recording Period

	Monthly Records $(A_1 + A_2)$ IL	*Two Fortnightly Records* $(B_1 + C_2)$ IL	*Difference* %
Selected foodstuffs:			
Sugar	2·28	2·70	17
Oil	2·66	3·20	18
Vegetables . . .	3·90	3·78	−3
Eggs	9·92	10·12	2
Meat	8·68	9·74	12
Fowl	13·62	14·04	3
All food . . .	116·9	123·2	5

For all food together, the fortnightly panels show an excess of some 5 per cent. over the monthly panel, which may not seem very serious.* The proportionate difference, however, varies substantially from commodity to commodity: it appears to be smaller for the frequently bought items (vegetables, eggs), and larger for the infrequently purchased items (sugar, oil). Further discussion of the factors at work is deferred to sections 2.4 and 2.5.

2.3. *Influence of Recording Y on Records of X*

We may next examine a question on which I have not hitherto seen any published information: what is the effect on the recorded expenditure on commodity *X*, of simultaneously keeping records of expenditure on commodity *Y*? This, of course, is related to the more general and very practical issue of whether it is better to ask each household in the sample for its complete budget, or to collect information on different sections of the budget from appropriately arranged subsamples.†

Now, as far as *non-food items* are concerned, the three panels in this inquiry recorded their expenditures for precisely the same calendar period of a month; if there is any difference between the results of panel *A*, on the one hand, and of panels *B* and *C* on the other, it can only be the consequence of differences in the requirement to record *food* expenditures.

The results that have been observed are set out in Table 4. It will be seen that panels *B* and *C* are in fact generally higher than *A*, the excess being especially marked for clothing and furniture, both of which tend to be large items that are infrequently purchased. On the other hand, the group of regular payments, heating and lighting, taxes, and rent, show only small excesses; these are especially interesting since the differences here will necessarily be largely due to a "recording bias", and only to a minor extent to an "expenditure bias".

Most surprising, perhaps, is the finding that the differences here are, on the whole, proportionately greater than those shown in the previous table. It may be that the pro-

* The difference is significant at the 1 per cent. probability level.

† The National Food Survey in this country, for example, collects information on food expenditures only.

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portionate biases are greater for non-foods, which are less frequently bought; or it may be that the other things that a household does while it keeps records of expenditure on a particular commodity are even more important than the length of time for which it is asked to keep records.

TABLE 4

Expenditure Recorded on Selected Non-food Items, According to Whether Food Expenditure was Recorded for Whole or Half of the Same Period

Item	*Food Expenditure Recorded for*			
	Whole Period	*Half Period*		
	A′	*B′*	*C′*	*Difference*[a]
	IL	IL	IL	%
Clothing	33	40	39	22
Medical care	5	5	6	10
Furniture	13	17	15	26
Household goods	16	16	19	11
Rent	5	5	4	−6
Heating and lighting	8	8	8	2
Education	10	12	11	12
Amusement	6	6	6	10
Transport	9	10	10	17
Tobacco	5	5	6	13
Personal services	3	4	4	16
Taxes	30	33	30	4
Insurance	6	7	6	10
Sundry	6	7	7	7
Total	155	180	177	13

Note: a. Calculated as $(B + C - 2A)/2A$. In calculating this percentage, the averages in the three other columns were taken to two further decimal places than shown here.

2.4. *Effects of Recording Fatigue*

One of the reasons for the observed tendency of recorded expenditures to decline with the length of inquiry period is recording-fatigue though, clearly, this is only one factor among many. Its effect can be examined a little more clearly by considering the quantities corresponding to the expected equality

$$A_2 = C_2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Both of these relate to expenditures during an identical period of time, namely, the second half of the calendar month; further, both sets of households during this fortnight were recording expenditures on both food and non-food items. The only difference between the two sets of households is that one set (A_2) had been recording food expenditure during the previous fortnight, and may have become tired; whereas the other set (C_2) had only begun its recording in the second part of the month, to which the present comparisons relate.

The results shown in Table 5 are in fact consistent with the hypothesis that fatigue is one of the factors at work: those households that have only just begun recording their expenditures on foods show a total that is 4 per cent. higher than do those that have been engaged in recording for the previous fortnight.* But the considerable variations between

* The difference for all food is significant at the 2½ per cent. level.

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the commodities suggest that this is not the whole story, though at the present state of knowledge any attempt to tell a fuller story must savour of rationalization. I therefore only pause to recall the possible influence of the frequency of purchase, and to note that sugar was rationed during the period of this survey and that there are grounds for thinking that the incentive for over-statement by C_2 was especially great there.*

TABLE 5

Recorded Expenditure on All Food in Second Half of Month, According to Activity in First Half of Month

	Records in First Half of Month		
	Kept (A_2) IL	*Not Kept* (C_2) IL	*Difference* %
Selected foodstuffs:			
Sugar	0·84	1·18	34
Oil	1·32	1·66	23
Vegetables	1·80	1·84	2
Eggs	4·96	5·02	1
Meat	4·12	4·74	14
Fowl	6·58	6·72	2
All food	57·2	59·3	4

2.5. *The Pure End-period Effect*

We must now examine the first half of the month and consider the quantities corresponding to the expected equality

$$A_1 = B_1. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

These quantities relate to two sets of households, chosen at random, which record their expenditures on both food and non-food items for precisely the same calendar period. Except for sampling fluctuations, the past and present activities of these households are the same; only their futures differ. Yet, as Table 6 shows, the households corresponding to B_1 recorded about 7 per cent. greater expenditure on all foodstuffs than did the A_1 households.†

This very remarkable finding, as far as I can see, can only be the result of households altering their recording habits as they approach the end of their recording period—a pure "end-period" effect. Group A_1 continues recording into the second half of the month; but group B_1, which ceases recording on the 15th of the month, raises its recorded expenditure—perhaps after examining the items it has recorded in the previous days and found them wanting.

2.6. *Discussion*

If the biases were of the same proportionate size for all commodities, there would be no need for alarm, as far as the cost-of-living index is concerned, since it is only the propor-

* Sugar was rationed by the calendar month, and there was a tendency to purchase the whole ration towards the beginning of the month on account of shortages. Consequently, a household in panel C that had bought sugar in the first half of the month, and was still consuming it in the second half, may have felt it appropriate to enter an expenditure on sugar when none had been incurred. For other commodities, which were more evenly purchased during the month, any such tendency must have been much weaker.

† The difference is significant at the 0·1 per cent. level.

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tionate pattern of expenditure that is required here. Unfortunately, it appears that the effects vary substantially from one commodity to another.

TABLE 6

Recorded Expenditure on Food in First Half of Month, According to Activity in Second Half of Month

	Records in Second Half of Month		
	Kept (A_1) IL	*Not Kept* (B_1) IL	*Difference* %
Selected foodstuffs:			
Sugar	1·44	1·52	5
Oil	1·34	1·54	14
Vegetables	2·10	1·94	−8
Eggs	4·96	5·10	3
Meat	4·56	5·00	9
Fowl	7·04	7·32	4
All food	59·7	63·9	7

In putting forward the above pieces of evidence it is difficult at this stage to do more than obtain a subjective impression of the various factors at work. My own view is that the factors leading to an overstatement of expenditures (the prompting effect of listing one's expenditures, the tendency to record items not bought precisely within the period, etc.) outweigh those leading to an understatement (forgetfulness, the discouraging effect of seeing, in black and white, one's total expenditure mount up, etc.). Consequently, I prefer the lower estimates which, in the case of the Israel inquiry, seem also to be more in accord with other evidence that was available. It would be interesting to hear other views, and of other experimental results in this field.

It may be that quite simple changes in procedure would eliminate much of the arbitrariness that has been observed in the results. For example, it may be better to ignore expenditures recorded in the first and last few days of the recording period; perhaps, also, more emphasis should be placed on recording payments made only precisely within the period; further, perhaps a cash reconciliation of incomings and outgoings should be attempted, either by the household itself, or later at the processing stage.

Though experiments in this kind of work are expensive, much could be done by combining them with routine inquiries carried out in the normal course of work. Planning is required; so is the determination to bring the results to the stage of publication; and so is the ability to bear the doubts that such experiments may cast on the precise validity of the other results of the inquiry.

3. *Some Conceptual Questions*

I turn now to consider certain conceptual problems on which, however, I have no statistical information to hand to indicate the magnitude of the implied alterations.

3.1. *Exclusion of Investment Expenditures*

A prior assumption that needs consideration is that the object of the index is to measure changes in the cost of maintaining a given level of consumption, rather than a given level of consumers' expenditure. The difference, of course, is the amount of net accumulation of consumers' fixed assets, consisting principally of houses, motor-cars, furniture, etc.

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While there is nothing in principle to prevent one taking the other view (that the index should also measure changes in the cost of that part of the basket consisting of net investment by consumers), this leads to a less convenient index number. The inconvenience arises from the fact that investment expenditures tend to be much more variable over time than the associated flows of consumption; the weight of durable commodities in the index might thus be unduly high or low, depending very much on the characteristics of the time at which the survey was held. Such an index number might thus be subject to the criticism that it was out of date, or no longer representative of current patterns of expenditure, rather sooner than would be the case for the consumption index number.

If our prior assumption is granted, it follows that it is in principle desirable to adjust the results obtained in an expenditure survey by excluding any net investment from the index-weights. The adjustment required will however generally not be substantial for most of the "consumers' durables" group, provided we are only interested in the average for a large group of households; for, though certain households will be incurring investment expenditures on these goods during the survey period, there will be others showing no expenditures but who are still deriving a stream of consumption services from past purchases. Except in periods of exceptional investment in durables, such as in 1955 in the United Kingdom, the expenditure on durables by a large group of households may not exceed their consumption by very much.

In making such adjustments, consideration must also be given to the possibility that households may be running down their stocks of certain goods, that is, dis-investing. This is especially likely to happen if some new commodity is introduced which partially replaces an older variety; the crude weight for the old and the new commodity combined may then not require any adjustment, but within this group the crude weight for the new commodity should be reduced and that for the old raised.

3.2. *Treatment of Housing Costs*

Houses are the most durable of consumer goods and the above remarks on investment are clearly applicable, and indeed are to some extent already reflected in current practice. The problems here however are wider, and need more fundamental examination.

The present practice in the U.K. is to calculate the *weight* of housing in the index on the basis of actual rent payments by those that rent their houses; and, for those that own their houses, on the basis—not of actual purchases (or purchase instalments)—but on the basis of an imputed rental value. The latter imputation is based on a valuation of the property for income tax purposes (under Schedule A), and this in turn is at present based on information for the year 1936–37 "with an adjustment to take account of changes in the average level of rents since before the war" (Cmd. 9710, para. 24). As far as movements in the *price* of housing is concerned, this is represented in the index by the rents paid for rented houses only. For owner-occupied dwellings the imputed rent is, in effect, assumed to move in the same proportion.*

Now, if the valuations for tax purposes in fact corresponded to the full rental value of the property obtainable in a competitive market and, further, if the market were in fact competitive, the present procedure would be substantially justified. But doubts arise on

* For clarity, I exclude here any mention of rates, repairs and insurance, which form part of housing costs, but for which the correct treatment is straightforward. [*Added in proof:* But see Professor Allen's contribution, p. 323.]

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both grounds. The taxation valuations of owner-occupied houses, even if brought up to date, would almost inevitably be too low on average (since even if the original assessments were unbiased estimates of the market value on average, they will subsequently be revised if the owner appeals; but, of course, he will only appeal if the original valuation was too high). And, even if rent restriction were entirely removed, it may be questioned whether rented and owner-occupied dwellings are in the same market: it is by no means inconceivable that the price of the one type of accommodation rises while the other falls. A braver attempt at including owner-occupied dwellings in the index ought therefore to be considered.

In passing, it is worth noting that during rent control an index of the true cost of living should include an adjustment to take into account the shortage of housing, similar to the adjustment usually advocated for rationed commodities. The theory for the latter is well developed (as in Rothbart's system of virtual prices); fortunately, with the present relaxation of rent control, it may be hoped the problem will disappear before any practical scheme is worked out!

If an attempt were made to include an imputed rent for owner-occupied houses, information would be necessary on the average capital value of a house, the average number of years for which the house can still be occupied, and the rate of interest to be used in amortizing its value over its lifetime. Denoting these by K, n, and r, the imputed annual rental value (calculated as an annuity with continuous compounding) would then be:

$$v = Kr/(1 - e^{-rn}). \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Changes in v may be attributed to the components as follows:

$$\frac{\delta v}{v} = \frac{\delta K}{K} + \frac{\delta r}{r} - \left\{\frac{\delta n}{n} + \frac{\delta r}{r}\right\} \frac{rn}{e^{rn} - 1}; \quad . \quad . \quad . \quad . \quad (5)$$

the last term is due to the finite life of the house, and tends to zero as rn increases.

The value of n is the most difficult to estimate but fortunately, as can be seen from (5), variations in its value have only a small effect on v; in practice changes in n could probably be ignored. Information need then be collected only on the prices of houses sold and on the rate of interest for long-term loans for houses; a given percentage change in either of these would lead to an equi-proportionate change in the imputed rent.

Among the many practical problems that would arise, that of obtaining comparable price quotations over time would be especially serious: it would be necessary to ensure that comparisons were made between houses of the same size, age, situation, etc. But it may perhaps be doubted whether these problems are so very much more serious than those in obtaining quotations for the rents of rented property over time.* And, however great the problems may be, it can hardly be doubted that many will find it useful to have systematic, if qualified, information on trends in the costs of owner-occupied and rented apartments.

3.3. *Hire Purchase Instalments*

The final question that I wish to raise this evening is also associated with durable goods (though it may apply elsewhere as well) and concerns the treatment of purchases on an

* Certain Building Societies in fact already compile series on the values of houses.

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instalment basis. In collecting information from households, the simple alternatives are either to request households to record the total purchase price ultimately payable for the item, and to omit from their budget any instalment payments for items acquired in the past; or, to ask them to record total payments in the period, irrespective of whether they relate to current, past or future acquisitions. Both methods, if applied to a large sample of households, would normally (that is, in the absence of substantial investment) yield averages of roughly the same order of magnitude. On grounds of sampling variability the second method is probably to be preferred, since payments are likely to vary less between households than are acquisitions; further, it is easier to prepare a cash reconciliation if emphasis is placed on recording payments, and this may lead to more accurate estimates.

The main point to be noticed here, however, is that the total payment consists of the cash price of the commodity plus interest and service charges on the outstanding loan. If there were any reason to suppose that movements in the prices of commodities (such as television sets) were parallel to that of service charges on loans, there would be no harm in treating the two as a single commodity. Otherwise, the household should be separately charged with the cost of maintaining such a loan, and "loan maintenance" is better treated as a separate item in the cost-of-living index.

The service to be measured would be that of having available a certain amount of purchasing power, an amount corresponding to that borrowed by an average household in the survey period. Hence, the service charge would be based on an amount of debt that would itself vary with the level of prices. Though the loan may in law be tied to the particular durable commodities bought (the television set, for example), it seems to me that the loan may equally be said to finance any other item of consumption: for this reason I suggest it varies with the index as a whole, rather than with the price of the particular durable commodity concerned.

Strictly, one should draw a further distinction here between the interest and service elements in the cost of maintaining a loan; but, as the text-books have it, "this is left as an exercise for the reader".

4. *Summary*

Much of what has been said above is, I am sure, familiar to those who have worked in this field though, I trust, none the less worth discussing. The issues I have raised are those where it seemed to me that further research was especially necessary; if I have ignored others which are of greater importance, no doubt other participants in this discussion will be happy to remedy the defect.

To summarize:

(*a*) Information on the weights for the index seem at present to suffer from a degree of uncertainty because of certain biases that arise in consumer reporting. These biases arise even if the information is based on account books kept by households. It seems desirable, therefore, to promote a series of investigations into these types of bias; these could most economically be carried out if integrated with routine inquiries, though provision should be made at the outset for ultimate publication.

(*b*) Further consideration should, I suggest, be given to the treatment of housing costs in the index, so as to include a more realistic imputed rent for owner-occupied houses.

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(*c*) Similarly, consideration should be given to the correct treatment of the costs incurred by households in maintaining their hire-purchase and other debt.

Acknowledgments

The arguments presented in this paper are largely the consequence of a visit in September, 1957, to the Central Bureau of Statistics of the Israel Government. I am grateful to Professor R. Bachi, Director of the Bureau, for arranging this visit, and for discussions with him and his colleagues, especially Mr. M. Sicron. The paper has also benefited from subsequent discussions with my colleagues at the National Institute of Economic and Social Research, London, to whom I am indebted for a number of improvements.

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Professor R. G. D. ALLEN: Dr. Prais has noted that practitioners in the field of price index numbers are largely in agreement on what the index should measure and on how, in general terms, it should be calculated. The "orthodox" index number of retail prices is of base-weighted (Laspeyres) form, price relatives being weighted according to the pattern of consumer expenditure in a selected base period. One merit of this formula is that the index is easily interpreted and explained to the "man in the street" as the changing cost of buying precisely the same "basket" of commodities from one month to another. This is important for the index is very widely used and it is essential that it should be generally accepted. It is known that the index tends to run high as compared with the true measure of price changes under the usual condition that there is a negative correlation between relative changes in price and quantity, i.e. on the assumption that housewives are good housekeepers, expanding their purchases where prices have risen less and contracting where prices have risen more than average.

One of the main problems of the base-weighted index is that, sooner or later, the basic pattern of expenditure gets out of date, the calculation of continuous price series for a fixed item of unvarying quality becoming practically impossible. The problem should be capable of an acceptable solution in practice, now that we have continuing consumer surveys. In the short period, over two or three years, the "orthodox" base-weighted index can be used. But, for comparisons over longer periods, some chaining or linking process can be adopted, the weighting varying as the consumption pattern changes. I am of the view that the best procedure is to link according to the Fisher "ideal" formula. I say that an *acceptable* solution should be possible for I am aware of the political requirements that the index should be published quickly and then not revised. This rules out, I think, any current form of chaining, for example by the use of the current year's consumption pattern as soon as it becomes available from the continuing consumer surveys. On the

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other hand, I think it is acceptable that back figures can be adjusted as a firm basis for long-period analysis.

Despite this, I think that further work needs to be done on alternative types of price index numbers, if only to supplement the "orthodox" index. Attempts have been made to determine the weights of the retail price index by principal component analysis of the series of price constituents of the index over a given period. Such attempts seem to be doomed to failure because of the very substantial auto-correlation in the price series. I have seen some (unpublished) work on the Canadian consumer price index by Professor T. I. Matuszewski of the University of British Columbia, which suggests that a principal component analysis of the series of *first differences* in the price constituents would be both valid and productive of a useful result. The analysis would determine the weights of an index of *price changes*, which would best describe the change in the component prices in a particular period, and this index might well be a useful supplement to the "orthodox" index of the *price level*. Some items (e.g. many foodstuffs) change frequently in price and in both directions; there might be a substantial increase in the price level in any period when many of these variable prices happen to be moving upward. But this may be no indication that prices generally are really beginning to rise, something which becomes clear when prices which normally do not change are actually beginning to increase. The index of price change would give a lower weight to items which are variable in price (e.g. food) and a greater weight to those which are less variable (e.g. durable consumer goods). It may well be that an indicator of this type is needed in addition to the "orthodox" price index in order to show up the basic price movements in the economy.

Dr. Prais has done well to ask us to think further about the practical aspects of the calculation of the retail price index in relation to its conceptual basis. First, a good deal of evidence has accumulated that there is a bias in the weighting of the index when the weights are derived from consumer surveys conducted on the interview or on the account book method. I am prepared to accept Dr. Prais's conclusion that expenditure is more likely to be overstated than understated in account books and that the excess is larger for items less frequently bought. I should like to see further evidence in support of his further conclusion that recording is more accurate if the household is accounting for all items of expenditure rather than a restricted number, an important point in conducting surveys. On the best length of time for the keeping of accounts it would seem that a balance has to be struck between the short period with its end-effects and the longer period when forgetfulness or recording fatigue sets in. As between a fortnight and a month, Dr. Prais's figures on meat expenditure in Israel, to take one of his examples, pose the problem of what is the best estimate of expenditure.

	Meat Expenditure (Israeli £)			
	Monthly Records		*Fortnightly Records*	
First fortnight . . .	A_1	4·56	B_1	5·00
Second fortnight . . .	A_2	4·12	C_2	4·74
Month	$A_1 + A_2$	8·68	$B_1 + C_2$	9·74

One estimate can be made on the joint assumption (i) that A_1 is unbiased and (ii) that B_1 and C_2 (though high) provide an unbiased estimate of the ratio of expenditure between the two fortnights. A_2 can then be raised to 4·32, giving an estimate of 8·9 for the month.

If Dr. Prais is right, then items which are bought less frequently are somewhat over-weighted in the present Ministry of Labour index of retail prices. The main items here are household durables and other manufactured goods, rather than foodstuffs and services, and it is these items which have increased less in price (at least since 1951). For this reason, therefore, the Ministry of Labour index may have been running a little low in the last few years.

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Secondly, there is the question of how to maintain appropriate price quotations from one month to another for particular items of expenditure. A critical group here, and certainly a topical one, is that of housing. For rented dwellings, the "price" series of rent and rates used in the present index is an average value and not a price quotation. It is obtained by taking the aggregate of rent and rates averaged over all rented dwellings, new houses coming into the reckoning and demolished or empty ones going out at successive periods. If the index is regarded as measuring the changing price (rent and rates) of houses of a fixed "quality", as required by the basic concept of the index, then it must be assumed that the stock of dwellings remains unchanged as regards age, size and general amenities. This is obviously not so and it may well be that the price index for housing has been running high, measuring the average housing cost for dwellings which are becoming newer and better.

For owner-occupied houses, the present index avoids the problem by first assigning rates, repair costs, etc. of these houses to the weights of the appropriate subgroup of housing, and then by spreading imputed rent over the whole weight of the housing group. (Dr. Prais is not quite correct in his comments on this.) The assumption is that imputed rent for owned houses moves parallel with all other housing costs, i.e. with rents of rented dwellings plus rates, repairs, etc., on all houses. This is obviously a very uneasy compromise. In any revision of the index it would clearly be an improvement to carry the imputed rent of owned houses as a separate housing subgroup, and to assign this weight to a price series designed to show changes month by month in the imputed annual rental value of a typical owned house. Dr. Prais has made a suggestion for such a series which seems practicable.

Equally, the present index has not provided a satisfactory treatment of "meals out" or indeed of the similar item of "fish and chips". Expenditure on meals out stated in the consumer budgets is first raised to allow for under-reporting, and then assigned half to the food weight and half for spreading over the whole set of weights in the index. Some attempt should be made to get price series for typical meals purchased in canteens and restaurants and so to carry "meals out" as a separate group in the index.

Finally, I should be glad to have suggestions on how to treat the weights and prices of seasonal items in the retail price index, a problem for which no satisfactory solution has yet been found. Seasonal price movements, fortunately, are not large when incorporated in the whole index, or even in the major price groups, but the problem is still worth tackling. Take eggs as an example of a seasonal commodity. Year to year price movements can be adequately handled by means of the weight assigned to eggs, i.e. recorded annual expenditure on eggs. Even here there is a slight difficulty since the price of eggs averaged over (say) twelve monthly quotations is not the same as the average price paid in a year, and the two averages may move a little differentially from year to year. The main difficulty is, however, to incorporate the month by month movements of egg prices in the monthly index. The trouble arises because consumption varies within the year, being negatively correlated with the seasonal price movements. The assumption in the present index is that the same quantity of eggs is bought month by month. This is not the case and the result is that upward seasonal movements are exaggerated in comparison with downward movements. It is difficult to see how the concept of a fixed *annual* basket of commodities (the basis of the index) can be reconciled with the need to include seasonal prices in the *monthly* index.

Mr. J. L. NICHOLSON: The first point which is likely to occur to anyone reading this paper is the absence of significance tests. It is sometimes a useful working rule to assume, as a first approximation, that all large differences are significant; and, since the sample contained 3,000 households, all the differences which are discussed in the paper as if they were significant probably are.

It is interesting to notice that the figures for the first fortnight are higher than those for the second fortnight. The figures for total food (in IL per household per month) are as

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follows:

	First Fortnight	*Second Fortnight*
A	59·7	57·2
B or *C*	63·9	59·3

I understand that most wage earners in Israel are paid once a month and not once a week which is the normal practice in this country. If they were paid at the beginning of the first fortnight, this might help to explain the decline in expenditure; towards the end of the month, people may be running short of money.

A general point which can be made in relation to all the problems which have been discussed is that conditions in Israel may be very different from what they are here; there may be different factors at work and the effect of any factor may be larger or smaller in Israel than it is in this country. One cannot necessarily draw general conclusions without taking account of the different conditions prevailing.

I was interested to see that Dr. Prais comes down, if rather gently, in favour of records which give a complete picture of expenditure, rather than confining them to one set of commodities. One would certainly prefer to have figures covering all items of expenditure since it is then possible to compare total expenditure with income and hence obtain a consumption, or savings, function; to estimate a consistent set of income-elasticities of demand, not forgetting the limitations of cross-section data for that purpose; to find out the degree to which different commodities are substitutable or complementary; and so on. Of course if it were found that information covering all items is less accurate than information limited to certain items, one would have to give up the struggle. But the evidence so far available suggests that the information covering all items of expenditure is, on the whole, the more accurate. Although it is asking a lot of people to fill up these long and complicated schedules every day for a fortnight or so, in this matter I remain an optimist. But Dr. Prais is wise not to commit himself too firmly.

Perhaps the most important conclusion to emerge from this paper is that we are dealing here with real problems. A fair amount of information has now been collected from various enquiries, most of which have been held in this country. Dr. Prais's results, those recently presented to this Society by Mrs. Cole and Mr. Utting, and the results of other enquiries all suggest that information collected from households, whether by interview or from record-books, is affected by various forms of bias. Further investigations are obviously called for, but Dr. Prais and the Israeli authorities deserve to be congratulated on having attacked the problem slightly more systematically than has been done previously, and for pointing the way to the further work which is needed.

I would also like to make a brief comment on the point raised by Professor Allen about the importance, in a cost-of-living index, of combining up-to-date weights with the concept of a fixed basket of commodities. It is the fairly obvious comment that it is possible to keep a forward-looking type of index (one which cannot be revised) up to date by the process of chaining, if one has a regular flow of up-to-date information about family expenditure. It can be argued on both theoretical and practical grounds, perhaps outside the scope of this discussion, that the weights of a price index should be regularly revised, so that they are always up to date. A continuous family budget survey, held year-in year-out, could provide the weights for a chain index in which each link covered a short period (e.g. one year), using Laspeyres formula. Without wishing to start a discussion on the chain type of index, I might just add that it avoids having to decide the point of time at which to make a break in the series and start a fresh index.

I would like to congratulate Dr. Prais on a very useful piece of work. Although he is a recognized authority in this field, one would not gather it from his modest address.

Mr. F. G. Forsyth: Dealing first with the arcane problem of biases in consumer surveys it may be helpful to consider their incidence in the two main methods of collecting the data, the personal interview and the account book system. In the interview method information is always sought for some past period and this period may be varied for

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different commodities. The account book method attempts to collect data on current transactions although there is always a small time lag between the actual transaction and the corresponding entry in the account book. We may consider the two methods in the following scheme:

Method	*Source of Bias*		*Likely Direction*	*Comparative Importance*
Personal interview	1. Memory defects	(*a*) End period effect	Upward	—
		(*b*) Confusing present with past	,,	—
		(*c*) Forgetfulness	Downward	—
	2. Reluctance		,,	—
Account book (supplemented by interviewer visits)	1. Memory	(*a*) End period	Upward	Less than interviewer method.
		(*b*) Confusing present with past	,,	Less than interviewer method.
		(*c*) Forgetfulness	Downward	Less than interviewer method.
	2. Reluctance		,,	Same.
	3. Fatigue		,,	More than interviewer method.
	4. Observation effect		,,	More than interviewer method.

Items 1, 2 and 3 give rise to what Dr. Prais has called "recording" biases while his "expenditure" bias must arise from the operation of item 4.

Of the memory biases Mr. Utting and Mrs. Cole (this *Journal*, 1956) drew attention to end-period effect, the tendency on the part of respondents to bring into any given period expenditure incurred in another period. When account books are used the effect of this can probably be lessened by ignoring the entries for the beginning and end of the period. In a paper to the Second Conference on Business and Economic Statistics held by the Study Section of the Society (March, 1958) Mark Abrams referred to some recent work on memory biases which suggested the conclusion that people tended to report present expenditure habits when asked about the past and thus, when living standards are rising, they overstate past consumption. Here again the account book system scores by concentrating on present expenditure. Finally, pure forgetfulness will impart a downward bias but is likely to be much more serious in the interviewer method. The net effect of memory bias is therefore likely to be upward according to Abrams and some confirmation of this is provided by the results of the Cambridgeshire survey in which the data were collected by personal interview and from which Mr. Utting and Mrs. Cole concluded that expenditure was probably overstated.

The reluctance of some respondents to give their true expenditures on certain commodities which probably affects recorded outlays on drink and tobacco seems just as intractable a problem whichever method is used to collect the data, but the account book system may suffer from the two additional biases arising from recording fatigue and from the very fact that a record of expenditure is being kept. Dr. Prais suggests that fatigue may partly account for the low results recorded by the A_2 sample in his Table 5. A commonly held view is that a good method of reducing one's expenditure is to keep a record of it and in so far as this is true the account book system will suffer from a downward bias which, as Dr. Prais states, will be impossible to deal with directly.

However the comparison between the two methods, although depressingly *a priori*, rather favours the account book system not only because it copes better with memory troubles but also because it seems to offer more hope of curing or at least estimating some biases by intelligent sample design. More work can be done in varying the recording periods in different parts of the sample as has been done in the Israel survey and the defects arising from memory and recording fatigue may be brought still further under control. The Ministry of Labour's 1953–54 household survey used this method and its organizers are satisfied that, on the basis of the admittedly rough checks they were able to carrry out, the items of expenditure are reasonably accurate except for those notoriously recalcitrant performers, drink and tobacco, and a few other items largely consumed outside the home.

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In view of the admitted shortcomings of index weights derived from surveys of consumers' expenditure it may be asked, firstly, whether it is possible to get more accurate weights by using other sources of information and secondly, how seriously is the index number affected by defective weights. Other sources are mainly statistics of production, imports and exports, wholesale and retail trade and receipts of Customs and Excise duties. From the point of view of an accurate measure of consumer expenditure most of these data are defective in one or more respects (see *National Income Statistics* produced by the Central Statistical Office, pp. 97–103) and have to be groomed by a number of more or less arbitrary assumptions even to provide checks on the estimates provided by surveys. The answer to the second question simply depends upon where the biases occur and the effect could be serious if the commodities involved had large weights. On the other hand, if there is any tendency for the size of the bias to vary inversely with frequency of purchase so that the large weight items like food, much of clothing, shelter and fuel are comparatively free from bias, then the index will not be too seriously in error.

Finally, turning to the problem of constructing weights which measure only consumption, although an index which excluded all investment expenditures might give more stable weights it would be rather more difficult to achieve than Dr. Prais suggests. The observed expenditure on some durable good may be taken to measure the consumption of that good only if the purchases are entirely for replacement. If the good is "new" and by no means in universal use the great bulk of observed expenditure will represent initial investment. The measurement of the consumption of such goods is no easy matter and the modification of the index weights seems hardly worth while.

Mr. KEMSLEY: I do not think it is a surprise to many people to be told that estimates of expenditure based on interview data are especially liable to bias. It is particularly easy to demonstrate the shortcomings of interview data. In fact, from some work we did in the Government Social Survey many years ago we concluded that the level of expenditure reported at an interview was decreased by the length of the period. This seemed to be a general finding although it was not possible to distinguish any definite variation from commodity to commodity. The same sort of result is reported in Dr. Prais's Table 1.

What may be more surprising is to find that this generalization applies to data recorded in account books. I am pleased to see that Dr. Prais has brought this out; it confirms something I have been saying from time to time since we made some experimental enquiries in the Social Survey. Preceding the Ministry of Labour Household Expenditure Enquiry in 1953, we made some preliminary surveys in 1950 and 1951. The experimental design of these surveys is rather simpler than that discussed by Dr. Prais. We found nothing quite so intriguing as the figures in Table 6 where expenditure is influenced by future activity, but we did find that the level of expenditure reported in the first few days was somewhere above the level reported in the remainder of the first week or in subsequent weeks. Thus an account book covering four weeks would show a lower rate of expenditure than one covering two weeks, a result consistent with Table 3. There is now ample evidence to show that reporting bias occurs in expenditure surveys but we do not yet seem to be able to explain how this occurs. Rather like the previous speaker, I think it is probable that the bias in an interview survey is produced in a different way from the comparatively smaller bias appearing in account books. It is common practice to attribute deficiencies of the interview technique to memory failure, but I shall be surprised if it is as simple as that. Data from account books should be free of this particular failing since if respondents do what they are asked they would record every payment as soon as it is made, but unless there is some sort of personal supervision by staff making periodic visits the records may be forgotten and not filled in until the end of the period or filled in only at infrequent intervals. Even with supervision, fatigue may cause a falling off in the recorded level of expenditure.

One or two speakers have made a point about the possibility that expenditure may be over-stated rather than under-stated. I am also of that opinion and I will mention one

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way in which this can occur. In order to avoid omissions the necessity of recording every payment must be emphasized. This is true of the sectional survey but it is more than ever true of a comprehensive enquiry. It is not always clear to respondents which transactions are properly payments and which they should ignore. Difficulties of this nature are likely to arise where the transaction is not clear cut. Goods bought on account, or through clothing clubs, or on hire purchase, or paid for by postal orders are a few examples. In these cases the respondent may become confused and in his desire to be helpful, especially when reminded that nothing should be omitted, enter transactions which should have been ignored. This sort of consideration will affect some types of commodity more than others. Probably such difficulties are best dealt with by getting a complete history of the whole transaction. In any case this emphasizes once again the importance of adequate supervision in the field work stage and the need for careful inspection at the editing and coding stages.

It has also been said that it is an open question whether there should be a sectional survey or a comprehensive enquiry. One of the advantages of a comprehensive enquiry is that instructions can be simplified. Respondents can be asked to record everything and, providing the description is adequate, the items in the books may then be classified in the office. However, in a sectional enquiry the respondent is often forced to decide whether particular items are within the definition, and border-line cases may very well be dealt with in different ways by different respondents. In food this is probably not a serious problem, since everybody knows what food is, and there will be few border-line cases. On the other hand in some durable goods it is possible to have many border-line cases and in surveys dealing with payments for services it is even more difficult.

For this and other reasons connected with the technical aspect of running a budget survey I feel that comprehensive enquiries are much more satisfactory.

Mr. EHRENBERG: I should first like to follow up one point made by Mr. Kemsley. Even in enquiries on a limited number of products, where these products can be specified in the account book or purchasing diary difficulties of definition will arise. We had a case the other day where a housewife did not record a purchase of so-called Red Tile Polish because she did not use the polish for polishing red tiles.

More generally, I want to say how extremely pleasant it is to have these data from the Israeli enquiry. It is courageous of them to have undertaken such an experimental investigation.

One point already raised is that biases may differ for slow-moving and fast-moving items, which would of course be a matter of great importance. But I am not sure that the data in this paper clearly support the conclusion that the slower moving items are possibly subject to bigger bias. Are eggs, for example, bought more frequently than meat? I do not know if that is so in Israel. Similarly, is oil a slow-moving item? In Italy we find that oil is often bought in penny numbers, several times a week; but Italy may differ from Israel in this respect.

Coming to the four types of effects which Dr. Prais discussed, I consider first the effects of length of enquiry and of fatigue. I doubt if there is sufficient evidence either here or in other enquiries to be certain that these effects exist. They may well be cases of initial over-reporting which Mr. Kemsley has already mentioned. This tendency to over-report initially is due to both recording bias and expenditure bias. Respondents may put down something which they had bought the week before, or they may realize that they are out of stock of some item and had better buy it.

Much of what has been said tonight is relevant only to enquiries of the kind often used for making index numbers and so on, where a recording period is usually very short, a fortnight or a month or two. The situation is quite different when one is talking about consumer purchasing enquiries covering much longer periods. In the continuous consumer panels which we operate these various points are not directly relevant because the Israeli enquiry covered only one month whereas we do not start using the purchasing records until after the first month or two. To return to the question of fatigue I do not

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think the evidence indicates fatigue as such. It is a little difficult to imagine people getting so fatigued in a fortnight of recording food items when they have to record non-food items for a month, and also recent experience both in this country and in Holland has shown that when consumer panels which have been running for years are compared with completely new panels there is no evidence of any general upward or downward bias.

The pure-end report effect is very interesting. Admittedly it is of no great importance in running continuous consumer panels, but knowledge about it would help us to understand the whole subject better, and one would like to do more about investigating it.

The effect of recording *X* on records of *Y* is very important. One does not know what is happening except that the stimulation of having to stop entering food items half-way through the period or having to start entering them re-focuses the respondent's attention on what he is doing and may make him a little more accurate in recording his non-food items. I am hoping to get some information on this point from an enquiry going on in Holland but no results are available at the moment.

One query I have about the Israeli experiment is that for both the non-food and food groups of items, and for practically all the individual items of food and other goods which are listed, the *A* sample is always lower than the *B* and *C* samples. This difference runs right through all the data and I wonder whether it has anything to do with the samples. Dr. Prais says that they are random samples but they are presumably not perfectly matched random samples. Their quality depends on the field work and how these different *A*, *B* and *C* methods were sold to the respondents. Did they know from the start that for *B* and *C* they would only have to report on food for a fortnight and that for *C* they should record their non-food items for a month but that later they would have to record something else as well? This kind of thing might have led to different co-operation rates in the three groups, resulting possibly in different average sizes of household which would affect the average rates of purchasing. I should like to know a little more about that.

Mr. W. M. GORMAN: Professor Allen suggested that we might avoid some of the difficulties implicit in budget surveys by basing our price indices on component analyses. I think this is a very dangerous idea. Underlying the whole notion of component analysis is the idea of a stable structure of the relationship between the variables in the analysis. It is difficult to believe that there is such a stable structure between prices in the absence of information about quantities. I cannot help feeling, for instance, that a component analysis of prices in the inter-war period would be badly confounded by the large change in economic structure, particularly on the supply side, in these years. It is quite probable that some important prices might turn out to have negative weights because, for instance, the trend in them was in a different direction from that in other prices. Would such an index be acceptable as a price index? Possibly even the principal component might have a zero correlation with some Laspeyre type price index. Would one feel that such an index had any value?

Quite a different situation, however, arises in cases where we have an economic theory from which a component analysis springs naturally. Such a case arises in the analysis of the demand for related goods. If one assumes that underlying this demand there are a small number of basic factors in terms of which a large proportion of the consumer's valuation can be expressed, then component analysis comes out naturally as a way of discovering these underlying factors. In some work which I have been doing recently I have used this technique, and have attempted to identify the specific factor, by assuming that the specific valuation of a particular good depends not at all on the consumption of the *other* related goods in the set being examined. It may be that component analysis has some part to play in such cases—or as an exploratory tool in cases where no simple economic theory exists—but I gravely doubt its value in determining general indices of consumer prices.

Mr. HOBSBAUM: In the Ministry of Labour Household Expenditure Enquiry of 1953 the enquiry was spread over the whole year, households kept budgets for three-week

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periods spread over the whole year and we have some information as to the distribution of expenditure for first week, second week and third week. We have done some preliminary work on this material and there is a lot more still to do. It is not easy at this stage to compare the kind of results we have got with the results to which Dr. Prais has drawn attention from the Israel Enquiry. For example, our first weeks were spread over the whole year, our second weeks were spread over the whole year and so were the third weeks. Another important difference may be that the records could be started on any day of the week except Saturday and Sunday so that first-week records are not the same days of the week. However, taking into account these differences the preliminary results show rather smaller variations than those shown in Dr. Prais's paper and I wonder whether this is because of the instructions given to householders and the arrangement of the book record? It would be very helpful if we knew more about the kind of instructions which were given to the householders in the Israel enquiry. Were the record books divided up into days or weeks; was there any possibility of wrong dating; were items or headings provided for each different commodity or groups of commodities in the record books and, if so, in what order; did interviewers scrutinize the records when collecting them and suggest possible omissions to householder? Our own records were, so to speak, self-contained for each week. Each week's record book had its own set of instructions and its own reminder check list at the end. This may be one reason why we do not find any "end-period" effect comparable with that illustrated in Dr. Prais's Table 6. I was interested to hear that Mr. Kemsley did not find any "end-period" effect in the 1950–51 pilot survey.

Mr. Forsyth suggested that in the light of the various biases about which we are learning more, we might have to look elsewhere for a basis for weighting price indices—presumably to national income statistics. I should point out, of course that when you have an index like the Index of Retail Prices, which does not cover the whole community but excludes certain categories such as high income or low income groups, it becomes particularly difficult to use these alternative sources. It seems to me that that does mean we are bound to stick to the consumer survey as the working basis for our weights, and thus the more that can be done to learn not only what these biases are but how we can avoid them, what instructions can be given, and how surveys can be organized to minimize them, the better.

Dr. PRAIS: Professor Allen asked what is the nature of the evidence, if any, that it is better to record the whole of the budget rather than only parts of it. Perhaps I could therefore say something by way of amplification of the material presented in Table 4. That table compares non-food expenditure by the three panels, and shows that panels *B* and *C* recorded higher amounts than did panel *A*. On further analysis (not reported in this paper) it appeared that the excess non-food expenditure by *B* and *C* occurred precisely in those fortnights in which they were not recording foodstuffs: in other words, when panels *B* and *C* were recording the whole budget, the amounts were much the same as for panel *A*; when they were recording only part of their budget, there was a tendency for the figures to come out rather higher. Further, the lower figures were in very much better agreement with information on incomes. I was therefore led to the provisional conclusion (and further evidence is certainly desirable on this matter) that when only part of the budget is recorded the tendency for bias is particularly marked.

Mr. Nicholson has noted that significance tests have not been applied systematically. Some of the smaller differences, I suspect, are not significant; but for "all food" significance tests are in fact included in the paper and the level of significance is indicated.

He has also noted that the first fortnight's expenditure tended to be higher than the second fortnight's, one of the reasons being that wages in Israel are paid monthly. Another reason is that many of the foodstuffs were rationed at the time of the survey; since the rations were also on a monthly basis there may have been a tendency for purchases to be made at the beginning of the month when the rations became due.

This brings me to some points raised by Mr. Ehrenberg. He asked, first, why oil was bought infrequently. The answer is that it was severely rationed at the time of the inquiry, so that it was more convenient to buy the whole ration at once. Secondly, with regard to

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meat: this is somewhat of a "luxury" in Israel, it is relatively expensive, and consequently is bought perhaps only once a week. Eggs, on the other hand, may be bought twice or three times a week.

I agree with him that part of the differences between panels *A*, *B* and *C* might conceivably have been due to differential response rates, but it is difficult to find any evidence to support this. Households were allocated to the three panels on a strictly random basis and, so far as I could tell from the results, there were no significant differences: the response rates were the same for the three panels, the average income was much the same, and so was the average household size.

The results quoted by Mr. Kemsley and Mr. Hobsbaum on the official Ministry of Labour survey are very welcome, as are those adumbrated by Mr. Ehrenberg; I hope that we shall one day have a fuller account of their researches.

Dr. PRAIS subsequently replied in writing as follows:

I should like to express my appreciation to the other participants in this discussion for their helpful and practical contributions. I am sure these will do much to promote further research into improving the accuracy of interview and recorded replies—an issue that is fundamental for the progress of social science.

Professor Allen has remarked that further work needs to be done on alternative types of price index numbers. Further work would indeed be useful, but personally I am not too hopeful. It is remarkable how little advance on conceptual matters has been made in the last half century; Wicksell, for example, had already reached what is the modern position in Chapter 2 of his book on *Interest and Prices* published in 1898 (English translation, Methuen, 1936). On what is to be done if the current and base weighted index numbers differ, he says (English edition, p. 13): "For practical purposes one might adopt some kind of average of the two figures—the arithmetic mean would be the simplest—but it would have a purely arbitrary significance." Professor Allen remarks that he prefers the geometric mean; this, I presume, is equally arbitrary, but perhaps has a certain "arithmetic" convenience. On the other hand I am more hopeful with regard to further work on matters such as the treatment of seasonal items in a cost-of-living index. A definitive and practical solution should not be too difficult to find, but not a little energy will be required to compare the various alternatives that have been put forward.

Mr. Hobsbaum asked about the instructions given to householders in the Israel enquiry: it will probably be sufficient if I say in reply that the instructions and procedures were very similar to those of the 1953 enquiry in this country. However, the households were also supplied with a "jotting pad" for entering the original records of their purchases, and these entries were later transferred to the final account book often with the help of the interviewer. The interviewer called every two or three days throughout the month of the inquiry to check that everything was going as it should.

As a result of the ballot taken during the meeting, the candidates named below were elected Fellows of the Society:

John Moffat Beattie
Brian Joe Lobley Berry
Douglas Basil Mullins Board
Robert Marcel Croizat
Gilbert John Ingram
Charles William Izzard
James McCardle Livingstone
Charles Louis O'Higgins
George Davidson Middleton
George William Morris
Owen Nankivell
Arjun Puri
Maurice Rothman
Wilfred Alec Shepherd.

Corporate Representative

Walter Kühnberg, *representing* The Financial Times Ltd.

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The Design of a Long-Term Sampling Programme to Measure the Hazard Associated with an Industrial Environment

By J. R. Ashford

National Coal Board, Pneumoconiosis Field Research

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1. *Introduction*

During recent years an increasing amount of attention has been given to the effect of industrial environments on the health of the workmen exposed to them. In some industries, such as cotton and pottery, the existence of a potential hazard has been recognized for a considerable period, whereas in others, such as atomic energy, the development of new methods and processes has introduced entirely new forms of risk. A common feature of many of the existing health problems in industry is that evidence of the hazard is slow to appear, although the cumulative effect of many years of exposure may be a permanent deterioration in the health of the men concerned. Because of this fact, the precise mechanism of action of the hazard is seldom fully understood and in some circumstances there is even a degree of doubt as to which particular aspect of the process or environment is the major contributory factor. This uncertainty is undesirable, not only for humanitarian reasons, but because the application of safety measures may be very costly, and unnecessarily stringent standards are inefficient.

In this country one of the most serious and long-standing of such problems is that of pneumoconiosis amongst coal miners. For many years pneumoconiosis has been legally recognized as an industrial disease and in the period between 1931 and 1949 some 30,000 men were found to have the disease and awarded compensation. During the 1950's the number of new certifications has remained at about 5,000 annually, out of a total labour force of some 700,000. Although the figures of certifications do not necessarily provide a reliable picture of the true prevalence of pneumoconiosis amongst the mining population, as they depend on the man voluntarily presenting himself for examination, it became increasingly apparent that the position was unsatisfactory and in 1952 the National Coal Board undertook a field research "in order to establish what environmental conditions should be maintained if mine workers are not to be disabled during the course of their work" (Fay, 1957; Fay and Rae, 1958). It was decided that this object could best be achieved by a study of the relationship between the incidence and progression of pneumoconiosis and the environmental conditions, amongst a sample of the mining population, lasting over a period of at least ten years. An essential component of this programme of research is the measurement of the environment experienced by the men under examination, and the design and

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organization of the environmental surveys, which are on a very large scale for a field study in industrial hygiene, have led to a number of statistical problems. Although the characteristics associated with this particular research have imposed some restrictions on the methods adopted, the procedures which have been evolved are of general application in the field of sampling surveys.

Before considering the sampling procedures in any detail it may be useful to give an account of the background of the problem. The condition of coal miner's pneumoconiosis (or "dust in the lungs") may be divided into two stages—simple pneumoconiosis, and complicated pneumoconiosis or progressive massive fibrosis (P.M.F.) (Gilson, 1957). Simple pneumoconiosis is a relatively benign condition, characterized by the presence of dust in the smallest passages (alveoli) of the lung which becomes surrounded by inert "scar" tissue in place of the normal lung tissue. This has the effect of reducing the reserve capacity of the lung, and thereby increasing the breathlessness normally associated with age (Gilson and Hugh-Jones, 1955). As a general rule simple pneumoconiosis even in its more advanced stages is not seriously disabling in men of less than middle age. Complicated pneumoconiosis or progressive massive fibrosis (P.M.F.) is, on the other hand, a much more serious condition, which is believed to be the result of the superposition of an infection (probably tuberculosis) on a background of fairly advanced pneumoconiosis. Unlike simple pneumoconiosis, P.M.F. appears to progress even when the man is removed from dust exposure and generally results in the progressive degeneration of the lung tissues.

Diagnosis of pneumoconiosis involves an X-ray film of the chest. Simple pneumoconiosis is divided on a quantitative basis into three categories, increasing in degree of abnormality and numbered 1–3, category 0 being reserved for films that are within normal limits. Complicated pneumoconiosis is divided into four categories of increasing severity—A, B, C and D.

The problem of pneumoconiosis amongst miners has been recognized since the Middle Ages and the theories which have been put forward about the subject are many and varied. Modern research, notably by the Medical Research Council in South Wales (Hart *et al.*, 1942), has established that exposure to airborne dust is the primary cause of the condition. Surveys at a number of collieries in South Wales between 1939 and 1942 revealed considerable differences in the prevalence of pneumoconiosis associated with differences in the characteristics of the coal seams and the adjacent rock strata. In the absence of any information about past dust conditions it was not, however, possible to determine any definite relationships between these various factors. The environmental studies in the Pneumoconiosis Field Research have therefore been directed primarily towards the measurement of the exposure of the miner to airborne dust. This is a complex problem as it is necessary to take account of the concentration, composition, size distribution and other factors associated with the dust clouds, as well as the amount of air breathed. Some narrowing of the range of measurement is permissible, since the majority of particles of more than about 5 microns in diameter do not penetrate into the alveoli. Attention can therefore be concentrated on the "respirable" fraction of the dust clouds and methods of measurement and analysis designed accordingly.

The main sources of dust production in the mines are the operations associated with the winning and transport of coal. These generally involve the "preparation" of the seams by means of cutting, boring and shot firing, the loading of the coal by hand or machine on to conveyor belts, and its subsequent transport to the shafts by way of trunk

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conveyors and rope or locomotive haulages. The driving of roadways for the transport of men and materials to the face and for the extraction of coal involves working in the rock strata adjacent to the coal seams and these activities also contribute to the prevailing dust concentrations. The effect of the ventilation systems, which are necessary to remove the explosive gases produced when the coal is worked, is that the airborne dust resulting from any particular operation may be transferred for considerable distances from the source.

In this country it is generally accepted that the most suitable instrument available for measuring the respirable fraction of airborne dust is the thermal precipitator. This instrument is based on the phenomenon of the dust-free space surrounding a hot body; the dust-laden air is drawn past a heated wire and the dust is deposited on to a pair of microscope cover slips placed in the vicinity of the wire. The samples so obtained are subsequently examined and counted under the microscope. The successful operation of the thermal precipitator requires the full-time attention of a skilled operator and the evaluation of the samples obtained with this instrument is a time-consuming operation. On the average, the examination of the dust samples taken during one shift occupies the operator's time for the whole of the next shift. In addition, the cost of the thermal precipitator and ancillary equipment is considerable.

2. *Selection of Men for Study*

The first stage in the research programme was the selection of a suitable sample of men for study. For various reasons, both administrative and psychological, it was considered that to choose some of the men working at a colliery and not others would be impracticable. The sample therefore consisted of all the men working at a number of different collieries and selection was made on the basis of the characteristics of the colliery, rather than on those of the men themselves.

Studies of the prevalence of pneumoconiosis revealed in the surveys carried out at various times by the Medical Research Council, which were mainly confined to South Wales, suggested that the composition of the airborne dust might well be a contributory factor in the causation of pneumoconiosis. In particular, it was thought that the rank of the coal seams (i.e., the type of coal—anthracite, steam coal, etc.), the proportion of non-combustible material present in the coal, and the sandstone content of the roof and floor strata, which all have a bearing on the composition of the airborne dust present at a colliery, were of primary importance. To obtain a representative sample of collieries which would cover all combinations of these characteristics, the sample was chosen in accordance with a factorial design, with four factors (average dust concentration on the coal-face, coal-getting shift; rank of coal; ash content; and sandstone content of roof and floor strata) each at two levels. As recent experience has shown that pneumoconiosis is present throughout all the British coalfields, and is not largely confined to South Wales as had at one time been supposed, it was considered that the sample of collieries should have a wide geographical distribution. Other considerations of practical importance, such as the need for the selected collieries to be reasonably large and to have an expected life of more than 20 years without major reorganization, were also take into account.

For the final selection of the sample, information was obtained about the relevant factors at the collieries operated by the National Coal Board which were considered to be

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suitable as regards size and future developments. The application of a random procedure did not prove practicable, owing to the need to obtain the agreement of the local management and workmen and to provide the best possible cover of mining conditions and methods of working. The selection of the sample was therefore made without recourse to any formal procedure and a total of 25 collieries was finally chosen to fill the 16 "boxes" in the factorial design.

Of the 25 collieries forming the sample, 6 are situated in South Wales, 5 in Scotland, 4 in Northumberland and Durham, 3 in Yorkshire and 1 each in Cumberland, Lancashire, North Wales, Staffordshire, Warwickshire, Nottinghamshire and Kent. All the major coal-fields in Great Britain, except Somerset and the Forest of Dean, are thus covered. At the present time there are some 35,000 men working at the selected collieries, representing a sample of about 1 in 20 of all mine workers employed by the National Coal Board. Apart from the fact that the sample contains a higher proportion of large collieries than occurs in the country generally, it is considered that the selected pits provide a representative cross-section of mining conditions and methods and that, although the choice was not strictly random, the results of the research will be of general application.

3. *The Man-shift as a Sampling Unit*

The main requirement of the environmental surveys is to measure the exposure to airborne dust of each of the men working at the selected collieries throughout the period of the research. Although the most appropriate index of the hazard has not been finally established, it is generally believed that the best single measure is probably provided by the total quantity retained in the lungs (Gilson, 1957). This is considered to be most appropriately expressed as a function of the integrated product of the dust concentration and the time at risk, the concentration being measured in terms of the number of particles of between 1 and 5 microns in diameter per unit volume. For this reason the main emphasis of the environmental surveys is directed towards the determination of the 1–5 micron concentration and hence the corresponding product of concentration and time at risk, which is denoted the "exposure index". In view of the uncertainty about this assumption, other measurements of the dust clouds, such as composition and size distribution, are also made, but these are at present considered to be of subsidiary importance. The difficulty and expense of obtaining and evaluating the thermal precipitator samples required to achieve this object is such that the measurement day-by-day of the dust exposure of the 35,000 men under consideration is manifestly impossible and it is necessary to employ some form of sampling procedure.

Almost all the operations at a colliery are performed by teams of men who work together at the same occupation and in the same working place. On any one shift there may be appreciable differences in the dust concentrations experienced by the individual members of these working teams (for example, owing to differences in position relative to the ventilation) but in the long run it is found that the average dust exposures are similar. At any one point in time it is thus possible to divide the colliery population into homogeneous strata or occupational groups, on the basis of occupation, place of work and shift. Unfortunately, however, men do not work in the same teams permanently. There are transfers from group to group and the groups themselves, which for the most part cover a particular occupation and working place, may also be of variable duration cor-

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responding to changes in methods of working and to the opening of new districts. Indeed, owing to the nature of the mining industry such changes are of frequent occurrence. The problem of the measurement of individual dust exposures may therefore be divided into two parts—the estimation of the exposure indices corresponding to the various occupational groups, and the recording of a history of the time worked and changes of occupational group for each individual.

The object of the environmental surveys is thus to measure the cumulative exposure of each individual over a considerable period of time. This is in contrast to the more familiar class of sample surveys, which are designed to provide information about a particular attribute in a population at a given point in time. The movement of individuals from stratum to stratum introduces a number of problems which are not encountered in conventional sampling surveys and in this sense the situation may be said to represent a generalization of stratified random sampling.

At any one colliery the distribution of the population between the various strata is constantly changing as men move from job to job and as old working places are abandoned and the replacements are opened. The strata themselves may differ in attributes other than dust concentration which are of relevance in the calculation of cumulative exposures. For example, strata which contain the more highly skilled workmen may tend to have less labour turnover than those containing unskilled workmen. Again, the attendance and overtime working of the men belonging to the occupations with the highest basic rates of pay may tend to be less than those of the less well paid occupations. Apart from these differences from stratum to stratum, there are wide individual variations, particularly in attendance and overtime working, between the men belonging to the same stratum, although the dust concentrations experienced during the course of their work are very similar.

For practical purposes the position becomes less confused if the sampling scheme is divided into a series of stages or surveys, each lasting for a comparatively short period. Within each survey the number of changes will be relatively small and it is possible to operate a normal stratified random sampling scheme. On general grounds it is desirable that the sampling units should form as small a proportion of the individual exposures as is practicable and the man-shift has been adopted as the basic sampling unit. The idea of using the individual man as a basis for sampling in environmental survey work was first introduced by Oldham and Roach (1952), who applied the principle in short period surveys of colliers at a number of the collieries examined by the Pneumoconiosis Research Unit of the Medical Research Council. The method has considerable advantages for the purposes of "research", as opposed to "control" sampling, and it was decided to extend the "random collier" concept to stratified random sampling in terms of man-shifts of the whole colliery population. Thus, in order to obtain a sample of the environment of any particular occupational group a random selection is made from the population of all the man-shifts worked in that group.

This procedure involves the random selection by name, from a stratified list of the colliery population, of the representative to be sampled and the random choice of the particular shift on which the sample is to be taken. There is inevitably a time lag between the preparation of the manpower list and the taking of the sample and, in case the chosen representative is absent or has transferred to another stratum in the meantime, the names of two reserves are also specified.

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When a particular sample is taken the dust investigator meets the chosen representative at the beginning of the shift at the shaft top and remains with him throughout the whole shift, taking a continuous series of dust samples with the thermal precipitator of the environment in the man's breathing area. Great care is taken to ensure that the presence of the investigator does not interfere with the representative's normal routine and the sampling instrument is placed in such a way that an accurate measurement is obtained of the air that the man is breathing. This procedure has proved to be extremely satisfactory in operation and, although the surveys have now been in progress for more than three years and approximately 10,000 sampling shifts have been completed, in no case has the investigator been prevented from sampling the representative. When the man chosen for sampling is working in potentially dangerous conditions it may be necessary to site the sampling instrument at some distance away, but situations of this kind are of rare occurrence. Thus the problem of non-response, which so frequently occurs in sampling surveys of human populations, has not been encountered in the environmental surveys.

In the mining industry the calculation of wages is based on the time worked day-by-day and records are maintained of the occupation, place of work, shift and attendance of all the men on the colliery books. The demarcation of the strata and sampling units at any particular time is thus a comparatively straightforward operation, as is the maintenance of a record of the time worked by each individual. Although the basic information is readily available, the only summaries prepared as a routine are those required for the weekly wages calculations. The recording of the individual working histories of 35,000 men on a weekly basis is obviously a vast undertaking and consideration was given to the possibility of employing sampling methods to obtain this information. However, preliminary studies of labour turnover and attendance revealed large individual variations, without any obvious basis for stratification, and it was decided that in spite of the quantity of data involved an individual record should be maintained for each man. This decision was based on the fact that the individual dust exposures are generated in terms of the product of the occupational group exposure index and the time at risk and that, compared with the estimation of the dust exposure indices, the expense of obtaining accurate records of time at risk is relatively small.

4. *Stratification of Man-shifts*

As in most applications of stratified random sampling the selection of strata is one of the major problems in the design of the sampling programme. It is generally accepted that the best characteristic for the construction of strata is the frequency distribution of the variable being measured or of some other quantity which is highly correlated with this variable. When the environmental surveys were first started this method could not be applied, as no suitable measurements of dust concentrations or of any variable correlated with dust concentrations were available.

The main principle on which the choice of the stratification was based is that the average of the shift exposure indices for any individual member of an occupational group, taken over any reasonably long period of time, must be virtually indistinguishable from the shift average exposure index for the group as a whole. In general this requirement is satisfied by men doing the same kind of work in the same place at the same time. This criterion provides a reasonable basis for grouping the majority of men, who work in teams, but in the case of occupations which cover men working singly or in small groups the

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rigorous application of the principle would lead to a multiplicity of strata each containing a very small number of men. From the point of view of the allocation of sampling effort it will be shown subsequently that the occupational groups should be as large as possible, which means that strata covering men who are subject to the same environment should be pooled. An additional disadvantage of having too large a number of strata is the difficulty of obtaining even one check sample for each group during a single survey. For these reasons, if there is no evidence of any appreciable differences between the working places, and the numbers of men of the given occupation who work together are small, grouping has been by occupation only.

Apart from the fundamental requirement that the shift exposure indices relating to the members of an occupational group should be constant both from man to man and in time, a number of subsidiary factors must be taken into account in the choice of the stratification. The thermal precipitator measures the environment in the vicinity of the man being sampled rather than the amount of dust actually retained in the lungs, and it is contemplated that a correction factor for breathing rate may have to be applied later to the calculation of individual dust exposures. This will obviously depend on the kind of work carried out and the pooling of strata containing different occupations has therefore been avoided. It is also necessary to take account of the fact that the exposure index calculated on the basis of the product of number concentration of airborne dust in the 1–5 micron size range and time at risk may not provide an absolute measure of the dust hazard and it is desirable that other aspects of the dust environment, such as composition and size distribution, should be constant within any given stratum.

The application of these principles of grouping is illustrated in the table on page 340, which shows the current stratification at a particular colliery and the corresponding exposure indices.

It will be seen that there is considerable variation from stratum to stratum and that, in general, the highest exposure indices are recorded for coal-face workers and the lowest for surface workers. Amongst the face workers who are grouped by occupation and place of work there are marked differences, both from occupation to occupation and from face to face for the same occupation.

The results of the surveys which have so far been carried out have confirmed that the initial grouping is, in general, satisfactory and comparatively few changes have since been made. For the most part the occupational grouping corresponds to a "natural" subdivision of the population but in certain circumstances there may be no *a priori* differences between two or more groups (e.g., if they cover the same occupation on different faces where the working conditions are similar), and the question arises whether or not the groups should be combined. A decision of this sort must involve a compromise between two conflicting considerations. On the one hand, the failure to combine groups which are subject to the same environmental conditions leads to an inefficient allocation of sampling effort (as will be shown later) but, on the other hand, the pooling of groups which are subject to different environmental conditions leads to bias in the estimates of the exposure of the men concerned. The two types of "error" are not directly comparable, nor is there at present any reasonable basis for assessing their relative importance. For this reason the combination of existing groups has generally been avoided, on the grounds that the absence of bias is preferable to a possible increase in the precision of the estimates of individual exposures.

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General Environment	*Occupation*	*Place of Work*	*No. of Shifts Sampled*	*Exposure Index (Arbitrary Units)* Mean	Standard Deviation
Coal face	Colliers	10's face	12	5,100	2,500
		16's face	15	6,100	1,400
		62's face	2	4,600	90
		65's face	11	6,700	1,500
	Colliers (Panzer)	21's face	8	6,800	2,300
	Power loader operators	17's face	6	12,000	12,000
	Rippers	10's face	4	2,600	1,400
		16's face	2	2,600	1,500
		17's face	4	4,600	2,800
		21's face	1	8,000	—
		65's face	5	4,500	1,300
	Cuttermen	10's face	2	3,500	1,200
		16's face	2	5,300	600
		17's face	3	4,900	910
		65's face	2	6,000	2,800
	Packers	10's face	7	3,100	680
		16's face	10	4,000	1,500
		17's face	9	4,100	1,900
		65's face	5	4,500	3,100
	Shotfirers	All faces	7	5,000	2,600
	Borers	,, ,,	2	3,500	90
	Face belt attendants	,, ,,	3	5,900	870
	Steel checkers	,, ,,	2	3,500	200
	Flitters	,, ,,	2	6,000	2,800
	Trainees	,, ,,	5	4,700	2,000
Elsewhere underground	Materials handlers	All districts	14	2,200	590
	Haulage men	,, ,,	7	1,000	540
	Officials	,, ,,	9	2,100	1,500
	Repairers	,, ,,	11	2,400	1,100
	Salvage	,, ,,	3	2,700	1,400
	Haulage engine drivers	,, ,,	2	300	150
	Pit bottom attendants	Pit bottom	7	630	230
	Electricians	All districts	6	2,700	940
	Loading point attendants	,, ,,	2	540	240
	Bricklayers	,, ,,	2	1,800	620
	Belt attendants and repairers	,, ,,	12	2,600	1,400
	Spillage attendants	,, ,,	7	2,900	1,100
	Fitters	,, ,,	2	2,800	460
	Loco drivers	,, ,,	2	1,600	140
	Development workers	,, ,,	6	5,400	2,100
	General workers	,, ,,	6	2,000	1,500
Surface	Screen attendants	Coal preparation plant	3	630	430
	Washery attendants	Washery	2	1,000	990
	Power house attendants	Power house	1	170	—
	Others	Surface (general)	9	200	120

5. *The Allocation of Sample between Strata*

As the measurement of dust concentrations is a costly operation there is an obvious need to determine how to allocate the available sampling effort between the various occupational groups at any particular colliery in the most efficient way. On general grounds it seems probable that the factors having a bearing on the problem will include the movement

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of individuals from group to group, the number of men working in any particular group and the accuracy of the occupational group exposure indices. In the case of conventional stratified random sampling it has been shown by Neyman (1934) that the optimum allocation (in the sense of making the variance of the estimated population mean as small as possible) occurs when the sampling fraction assigned to any particular stratum is proportional to the standard deviation. When the allocation of sampling effort in the environmental surveys is considered the situation is rather different, both from the point of view of the basic purpose of the sampling programme and of the criteria for optimum allocation.

The aim of the environmental surveys is to estimate the individual dust exposures over the period of the research, and the population mean, either at one point in time or over the whole research, is not of primary importance. Thus, it is desirable that the accuracy of the estimates of the individual exposures should be as high as possible. When the correlation of environmental conditions with the progression of pneumoconiosis (as measured by periodic medical examinations) is finally carried out, an estimate will be made of the dosage-response relationship between dust exposure and pneumoconiosis. From the point of view of recommending "safe standards" for the environment in the coal-mines in the future, attention will probably be concentrated on one particular part of the dosage-response curve (possibly between the 5 and 20 per cent. points) and the greatest amount of information will be derived from men whose dust exposures are such as to place them in this region. Unfortunately it is not possible to forecast in advance which men will be concerned, in view of the uncertainty about the exact form of the dosage-response relationships and the future dust exposures of the men at present working at the selected collieries. On balance, therefore, it is considered that each individual should be regarded as being of equal importance. In these circumstances the optimum allocation of sampling effort is that which makes the average accuracy of the estimates of individual exposure, taken over all the men who remain at the colliery throughout the research, as high as possible. In general terms this criterion would be applicable in any situation where the attribute under consideration is directly related to the product of the rate of application of a particular substance and time.

For any one occupational group, say G_i, the true exposure index will have a certain value X_i, given by the average of the shift exposure indices taken over all the members of the group and over all shifts during which the group is in existence. This quantity is estimated by $\hat{X}_i$, the sample mean. As a result of the randomization of the procedures for the selection of individuals for sampling, the mean of the distribution of the individual shift exposure indices is the occupational group exposure index, X_i. The distribution will have characteristic variance σ_i^2, which will depend on the variability of environmental conditions within the group. If the number of sampling shifts assigned to the group G_i is denoted by S_i, the sampling distribution of $\hat{X}_i$ will have mean X_i and variance σ_i^2/S_i.

Let us consider the exposure of a particular individual, say O_j, who remains at the colliery throughout the period of the research. The number of shifts spent by this man in group G_i will be denoted by t_{ij}*. (If the man never belonged to the group G_i, then $t_{ij} = 0$). Further, let the true exposure of the individual O_j during the period of the research be denoted by Y_j.

Now, Y_j will be made up of the sum of a number of increments representing the expo-

* It is assumed that t_{ij} is accurately known.

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sure of O_j in each of the groups to which he has belonged during the research, i.e.,

$$Y_j = \sum_i X_i t_{ij},$$

where the summation is taken over all the occupational groups in existence at any time during the research. The true exposure, Y_j, may be estimated by the quantity $\hat{Y}_j$, where

$$\hat{Y}_j = \sum_i \hat{X}_i t_{ij}.$$

By virtue of the properties of the distribution of X_i, the expected value of $\hat{Y}_i$, is given by

$$\begin{aligned} E(\hat{Y}_j) &= E(\sum_i \hat{X}_i t_{ij}) \\ &= \sum_i E(\hat{X}_i)\, t_{ij} \\ &= \sum_i X_i\, t_{ij} \\ &= Y_j. \qquad . \qquad . \qquad . \qquad . \qquad . \qquad . \qquad (1) \end{aligned}$$

Hence the estimate of total exposure is unbiased.

Further, we have

$$\begin{aligned} \text{var } \hat{Y}_j &= \text{var } (\sum_i \hat{X}_i\, t_{ij}) \\ &= \sum_i (t_{ij}^2 \text{ var } \hat{X}_i) \\ &= \sum_i \frac{t_{ij}^2\, \sigma_i^2}{S_i} \qquad . \qquad . \qquad . \qquad . \qquad . \qquad . \qquad (2) \end{aligned}$$

The average value of the variance of the individual estimates of total exposure may be derived as follows:

$$E(\text{var } \hat{Y}_j) = \frac{1}{N} \sum_j \sum_i \frac{t_{ij}^2\, \sigma_i^2}{S_i},$$

where N is the total number of men who remain at the colliery throughout the research.

$$\begin{aligned} &= \frac{1}{N} \sum_i \sum_j \frac{t_{ij}^2\, \sigma_i^2}{S_i} \\ &= \frac{1}{N} \sum_i \frac{\sigma_i^2}{S_i} \sum_j t_{ij}^2 \qquad . \qquad . \qquad . \qquad . \qquad . \qquad (3) \end{aligned}$$

The expression $\sum_j t_{ij}^2$ represents the square of the period spent in group G_i, summed over the whole of the population who remain at the colliery throughout the research. Precise measurement of this quantity for any particular occupational group would be extremely complicated but it is possible to derive an approximation in terms of the product

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of the square of the average period spent in the group taken over all individuals who have at some time worked in the group, and the total number of men who have worked in the group and are likely to remain under observation at the colliery.

The average value of t_{ij} (excluding all $t_{ij} = 0$) will depend on a number of factors. On general grounds it is apparent that the longer the duration of the group itself, the greater will be the average period spent in the group. The attendance of the men working in the group and the amount of movement to and from the group will also have a bearing on the average value. The average time spent in an occupational group which contains men who are good attenders and who work regularly in that group will obviously be greater than the corresponding value for a group which contains men who are poor attenders and who may be more liable to be transferred to other jobs. Thus the average value of t_{ij} may be expressed as the product of the duration of the group (d_i) and a combined labour turnover and attendance factor, (b_i^2), where $b_i \leq 1$. Similarly, the number of men who have at some time worked in the group and who will remain at the colliery throughout the research will be given approximately by the product of the average number of men belonging to the group (n_i) and a labour turnover factor, (c_i^2), where c_i measures both the movement from group to group within the colliery and to and from the colliery. If it could be assumed that all men belonging to the group would remain at the colliery throughout the research it would be possible to assert that $c_i \geq 1$, but movement from a colliery may be greater amongst the unskilled workmen than amongst the skilled and it is therefore necessary that c_i should also take account of this factor. For these reasons the expression $\sum_j t_{ij}^2$ is approximately equal to the product of the square of the duration of the group, the average number of men in the group and an attendance and labour turnover factor $a_i^2 (= b_i^4 c_i^2)$.

Thus we have

$$\sum_j t_{ij}^2 = a_i^2 n_i d_i^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

and, from (3),

$$E(\text{var } \hat{Y}_j) = \frac{1}{N} \sum_i \left[\frac{a_i^2 n_i d_i^2 \sigma_i^2}{S_i} \right] . \quad . \quad . \quad . \quad . \quad (5)$$

At any particular colliery N is a constant and the problem reduces to that of finding the minimum value of the expression

$$\sum_i \left[\frac{a_i^2 n_i d_i^2 \sigma_i^2}{S_i} \right]$$

with respect to S_i, subject to the restriction that the total sampling effort

$$\left[\sum_i S_i = S \right]$$

is predetermined (depending on the number of investigators available at the colliery and the rate at which they work). A solution may readily be obtained by the method of Lagrange's Multipliers as follows:

$$\frac{\partial}{\partial S_k} \left[\sum_i \frac{(a_i^2 n_i d_i^2 \sigma_i^2)}{S_i} + \lambda \left(\sum_i S_i - S \right) \right] = 0 \quad . \quad (6)$$

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and

$$\sum_i S_i = S \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where λ is an undetermined multiplier.

Hence

$$-\frac{a_k^2\, n_k\, d_k^2\, \sigma_k^2}{S_k^2} + \lambda = 0$$

i.e.,

$$S_k = \lambda^{-\frac{1}{2}}\,(a_k\, n_k^{\frac{1}{2}}\, d_k\, \sigma_k). \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Thus, for the optimum allocation of sampling effort the available sampling shifts must be distributed in such a manner that the total number of samples assigned to a particular group is proportional to the product of the duration of the group, the standard deviation of the individual shift exposure indices for the group, the square root of the average number of men belonging to the group, and a group labour turnover and attendance factor.

For any particular group, the efficiency of the sampling scheme may be measured by the required number of sampling shifts per man per unit time. If this quantity is denoted by u_k we have, from above,

$$u_k = \frac{\lambda^{-\frac{1}{2}} a_k n_k^{\frac{1}{2}} d_k \sigma_k}{n_k d_k}$$

$$= \lambda^{-\frac{1}{2}} a_k n_k^{-\frac{1}{2}} \sigma_k. \quad . \quad . \quad . \quad . \quad . \quad (9)$$

Thus, for given a_k and σ_k, the larger n_k (the number of men belonging to the group), the more efficient the sampling scheme will be.

It follows that, if there are no real differences in the attendance, labour turnover and environmental conditions experienced by a number of occupational groups, it is advantageous to pool the groups, rather than to keep them separate.

In view of the movement of manpower from group to group and the changes in the groups themselves, it is necessary to carry out the environmental sampling in a series of short stages or surveys. The best length of survey varies from colliery to colliery according to the individual circumstances and the choice of a particular period must be governed by three main considerations. First, it is necessary to cover the occupational groups which are of short duration; if the survey period were too long any groups which were not in existence at the beginning and had closed before the end of the survey would not be sampled. Secondly, it is necessary to obtain information about temporal trends in the environmental conditions relating to the groups which are of longer duration. This may best be carried out by having a comparatively short survey period so that the samples for each group are relatively uniformly distributed in time. On the other hand, a third consideration is that the survey should be sufficiently long for at least one sample to be obtained for the majority of the occupational groups. At any particular colliery these factors must be balanced against the available sampling effort and current number of groups. In practice the prevailing conditions are such that a survey period of about six months has been found to be satisfactory at the majority of collieries.

On the basis of the expression (8) for the optimum number of sampling shifts to be assigned to any occupational group throughout the research, it is possible to derive a procedure for the allocation of sampling effort during a particular survey. The most

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appropriate opportunity to review the sampling position occurs at the conclusion of each survey, when estimates of a_i, n_i, d_i and σ_i, relating respectively to the attendance and labour turnover, average number of men, duration, and standard deviation of the shift exposure indices, will be available for the current occupational groups. From these quantities a "sampling index", $w_i = a_i \sigma_i d_i n_i^{\frac{1}{2}}$, may be calculated for each group. For the most efficient allocation of sampling effort equation (8) shows that the total number of sampling shifts assigned to the group throughout the whole period of the research will be given by Lw_i, where L is a constant for all groups, depending on the sampling effort available at the colliery.

If t_i sampling shifts have already been obtained for a particular group, the additional number of shifts required is $[Lw_i - t_i]$. To give the best opportunity of examining any possible temporal trends these additional shifts should be spread as evenly as possible over the remainder of the period that the group is in existence. The proportion, p_i, which must be taken during the coming survey will therefore be given by the reciprocal of the number of new surveys which will be carried out before the group G_i closes. The number of sampling shifts required during the next survey (u_i) may thus be obtained by means of the expression

$$u_i = p_i (L w_i - t_i) \quad . \quad . \quad . \quad . \quad . \quad (10)$$

The appropriate value of L may then be calculated by summing the values of u_i over all the groups in existence during the coming survey and equating this value with the total sampling effort available for the survey. Thus, if the total number of sampling shifts which can be covered during the survey is denoted by U, we have

$$U = \sum_i p_i (L w_i - t_i)$$

i.e.,

$$L = \frac{U + \sum_i p_i t_i}{\sum_i p_i w_i} \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

Hence for each group it is possible to calculate the number of shifts required during the particular survey by means of the expression (10).

This procedure has the advantage that the information available may be reviewed after each survey and any necessary adjustments to the allocation of sampling effort may then be made. Factors such as the duration of the occupational groups are by no means accurately known, in view of the uncertainty of mining conditions and the rapid progress in the mechanization of the methods of working which is now being made. In the case of groups which will continue indefinitely it is necessary to make some assumption about the duration of the research, which is problematical, depending as it does on the time taken to reach sufficiently accurate conclusions. The suggested procedure allows for changes in this estimate and any possible over- or under-sampling in previous surveys may be corrected.

Summary

As part of the Pneumoconiosis Field Research of the National Coal Board, a programme of environmental studies is being carried out to measure the exposure to airborne dust of a sample of some 35,000 mine workers over a period of at least ten years. This sample

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represents the whole of the underground and surface populations at twenty-five collieries, selected to provide a representative cover of the factors which are considered to be of importance in the causation of pneumoconiosis. The difficulty of obtaining reliable estimates of airborne dust is such that the measurement day by day of the exposure of the 35,000 men under consideration is manifestly impossible and it is necessary to employ some form of sampling procedure. At any one point in time it is possible to subdivide the population at any particular colliery into homogeneous strata or occupational groups, on the basis of occupation, place of work and shift. However, men do not remain in the same strata permanently; there are transfers from stratum to stratum and the strata themselves may exist for only a limited period. The problem of the measurement of individual dust exposures may therefore be divided into two parts—the estimation of the levels of exposure corresponding to the various strata and the recording of a history of the time worked and movement from stratum to stratum of each individual. The object of the environmental surveys is thus to measure the cumulative exposure of each individual over a considerable period of time, in contrast to the more familiar class of sample surveys which are designed to provide information about a particular attribute of a population at a given point in time. The movement of individuals from stratum to stratum introduces a number of additional problems and in this sense the situation may be said to represent a generalization of stratified random sampling. By dividing the sampling programme into a series of stages or surveys each lasting for a comparatively short period, it is possible to operate a conventional stratified random sampling scheme within each survey. The man-shift has been adopted as the basic sampling unit and, to obtain a sample of the environment of any particular stratum, a random selection is made from the population of all the man-shifts worked by the members of the stratum. When a particular sample is taken the dust investigator meets the chosen representative at the beginning of the shift at the shaft top and remains with him throughout the whole shift, taking a continuous series of dust samples. The choice of the stratification is considered in detail and it is shown that the grouping should be carried out in such a way that the average of the shift exposure indices for any individual belonging to a given stratum, taken over any reasonably long period of time, should be virtually indistinguishable from the average of the shift exposure indices for all the members of the stratum, taken over the whole of the period during which the stratum is in existence. In general this requirement is satisfied by men doing the same kind of work in the same place at the same time. The allocation of sampling effort is considered and it is shown that the optimum number of sampling shifts assigned to a stratum should be proportional to the product of the duration of the stratum, the standard deviation of the shift exposure indices for the stratum, the square root of the average number of men belonging to the stratum and a stratum labour turnover and attendance factor. On the basis of this criterion a procedure is derived for the allocation of sampling effort during any particular survey.

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original selection of collieries was based. This paper is published by permission of the National Coal Board.

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Wholesale Prices in 1957

By the Editor of "The Statist"

Method of Construction of the Index Numbers

The following table illustrates the method of construction of the index numbers. The index numbers here given are based on the average prices for the eleven years 1867–1877. Take, for instance, the *Gazette* price of English wheat:

	s.	d.			
Average, 1867–77 . . .	54	6,	average point.		
,, 1914 . . .	35	0,	64 or 36 per cent.	*below*	the average point.
,, 1930 . . .	80	7,	148 ,, 48 ,,	*above*	,, ,,
,, 1936 . . .	53	3,	98 ,, 2 ,,	*below*	,, ,,

The individual index numbers, therefore, represent simple percentages of the average point. The 45 articles are grouped in six categories. They are shown in detail in Table 5.

The general average is drawn from all 45 descriptions, which are treated as of equal value, and is the simple arithmetic mean.

Table 1

Construction of Index Number for 1957

1867–77 = 100

	Number of Commodities in Index	*Total Numbers*		*Index for* 1957
		1867–77	1957	
General average	45	4,500	16,920	376
Food	19	1,900	5,951	313
Vegetable food	8	800	2,356	295
Animal food	7	700	2,402	343
Sugar, coffee and tea . . .	4	400	1,193	298
Materials	26	2,600	10,969	422
Minerals	7	700	3,991	570
Textiles	8	800	3,260	408
Sundry materials . . .	11	1,100	3,718	338

Table 2

*Annual and Monthly Index Numbers**

1867–77 = 100

	Year	*Jan.*	*Feb.*	*Mar.*	*April*	*May*	*June*	*July*	*Aug.*	*Sept.*	*Oct.*	*Nov.*	*Dec.*
1902–1911 .	74	73·9	74·0	74·2	74·5	74·9	74·5	74·5	74·3	74·3	74·5	74·4	74·8
1912–1921 .	148	145·2	146·3	146·9	147·7	147·9	147·2	147·9	149·2	149·0	149·3	149·1	149·2
1922–1931 .	120	122·7	123·0	122·5	122·0	121·0	119·3	119·0	118·0	118·0	118·6	119·2	118·5
1932–1941 .	97	96·0	96·5	96·5	96·8	97·3	96·9	97·5	97·8	98·7	98·8	99·4	101·4
1942–1951 .	231	221·4	224·1	224·6	227·3	228·0	227·1	226·7	227·3	231·2	235·3	238·5	241·2
1943 . .	155	153·3	153·2	154·0	154·9	155·6	155·4	156·0	154·4	154·6	153·7	153·9	153·9
1944 . .	160	154·9	155·1	155·7	157·9	159·5	160·0	161·9	161·5	160·1	159·6	160·3	160·7
1945 . .	164	161·4	161·5	162·1	162·4	164·0	166·7	165·2	163·2	162·7	162·7	162·7	163·2
1946 . .	186	165·6	166·2	166·6	168·4	169·3	169·7	174·4	175·7	180·5	196·2	198·1	200·5
1947 . .	230	206·7	209·6	212·1	215·9	216·5	218·1	223·4	225·9	228·9	236·0	240·8	246·5
1948 . .	260	250·5	253·4	256·0	256·4	260·0	263·9	260·4	260·1	258·1	261·5	263·8	266·0
1949 . .	274	267·6	266·8	265·4	272·0	269·2	267·2	263·3	262·5	285·7	291·5	293·7	297·4
1950 . .	324	300·2	301·0	301·7	305·2	308·3	310·3	317·7	332·1	335·7	349·2	363·1	372·3
1951 . .	402	404·8	420·5	419·3	425·7	420·6	405·2	394·5	387·9	395·9	392·0	396·9	399·7
1952 . .	380	403·5	394·8	384·4	377·5	372·2	379·5	374·4	370·4	369·9	368·9	369·4	372·9
1953 . .	366	374·5	375·2	374·9	370·3	368·1	367·5	361·3	357·9	357·0	357·3	357·3	360·0
1954 . .	361	359·8	361·2	366·1	366·5	363·9	361·2	355·5	353·5	353·7	356·8	362·9	371·7
1955 . .	370	378·0	376·5	375·8	370·2	370·1	370·1	373·1	363·9	366·8	372·4	370·9	374·1
1956 . .	384†	372·6	379·2	379·8	385·9	383·8	379·7	374·2	373·4	376·2	384·1	393·1	402·9
1957 . .	376	398·0	392·7	389·1	390·7	381·8	377·3	375·1	364·6	364·3	361·4	364·0	364·7

* The average of the twelve monthly figures of each year does not necessarily coincide with the annual figures, as the latter are calculated mostly from the average of 52 weekly quotations, while the former are based on end-of-the-month prices.

† Amended.

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TABLE 3

Annual and Quarterly Index Numbers for Groups

1867–1877 = 100

	All Groups	Food				Materials			
		Total	Vegetable Food	Animal Food	Sugar, Coffee and tea	Total	Minerals	Textiles	Sundry Materials
1902–1911	74	71	66	88	49	77	90	70	74
1912–1921	148	146	136	167	100	154	166	151	148
1922–1931	120	115	101	150	80	122	137	128	110
1932–1941	97	88	84	116	49	103	131	94	91
1942–1951	231	172	201	169	121	273	300	306	232
1943	155	138	156	156	72	167	187	166	156
1944	160	137	152	156	73	178	197	182	161
1945	164	139	155	156	78	182	209	189	159
1946	186	140	155	154	88	219	239	231	198
1947	230	156	191	149	100	284	304	295	263
1948	260	171	217	155	107	324	368	348	279
1949	274	201	240	185	151	328	382	356	273
1950	324	235	268	204	224	390	416	491	299
1951	402	265	305	227	249	502	516	639	394
1952	380	294	336	285	227	443	532	475	363
1953	366	325	365	317	261	399	473	430	329
1954	361	326	330	325	320	386	476	429	298
1955	370	329	313	367	281	400	529	410	310
1956	384*	326	319	351*	282	428	581	400	352
1957	376	313	295	343	298	422	570	408	338
1956 1st Quarter	377·2	324·7	323·2	361·5	263·4	415·7	579·4	392·5	328·5
2nd ,,	383·1	335·5	342·9	362·6	267·7	418·8	565·6	391·4	342·9
3rd ,,	374·6	312·2	300·3	351·1	268·1	420·2	578·7	390·0	338·8
4th ,,	393·4	332·3	309·8	360·8	328·1	438·1	595·7	419·5	351·3
1957 1st Quarter	393·3	328·9	304·3	351·2	339·1	440·4	594·9	423·3	354·4
2nd ,,	383·3	321·2	285·9	372·6	302·4	428·5	571·2	429·3	337.2
3rd ,,	368·0	304·0	292·3	336·8	270·7	414·7	560·5	404·8	329.0
4th ,,	363·5	304·4	298·7	326·7	276·8	406·5	549·2	387·4	329·6

* Amended.

TABLE 4

Monthly Index Numbers for Groups

1867–1877 = 100

	Year	Jan.	Feb.	Mar.	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
1956													
Food:													
Total	326	323·2	325·5	325·4	344·1	335·4	326·4	311·7	309·7	315·3	318·6	330·3	348·1
Vegetable food	319	313·3	320·1	336·1	358·7	344·5	325·5	307·2	300·1	293·5	298·7	313·5	316·1
Animal food	351	367·6	361·9	354·9	365·3	363·7	358·8	345·5	349·1	358·7	345·6	349·9	386·9
Sugar, tea and coffee	282	265·3	272·5	252·3	263·7	267·8	271·7	261·8	259·8	282·8	311·0	329·5	343·7
Materials:													
Total	428	408·8	418·8	419·6	418·6	419·1	418·6	419·9	419·9	420·7	432·0	439·1	443·1
Minerals	581	569·6	591·8	576·7	565·3	565·7	565·6	575·6	580·2	580·3	603·5	589·8	593·8
Textiles	400	393·0	390·9	393·7	392·0	394·5	397·8	393·1	394·7	392·3	403·8	427·6	427·0
Sundry	352	317·9	329·0	338·6	344·7	343·7	340·3	340·3	336·3	339·9	343·4	351·5	359·0
1957													
Food:													
Total	313	336·2	331·7	318·9	326·6	319·6	317·6	313·0	299·7	299·4	296·5	310·4	306·4
Vegetable food	295	313·3	305·7	293·9	288·9	277·9	290·9	302·6	288·3	286·0	293·1	301·9	301·2
Animal food	343	357·1	353·7	342·8	378·1	377·7	362·1	348·1	332·3	329·9	315·6	337·5	327·0
Sugar, tea and coffee	298	345·3	344·7	327·2	312·3	301·7	293·3	272·5	266·5	273·0	270·2	279·7	280·5
Materials:													
Total	422	443·3	437·3	440·5	437·5	427·1	421·0	420·3	412·0	411·7	408·8	403·3	407·4
Minerals	570	593·5	594·1	597·2	588·5	567·3	557·8	565·8	553·0	562·6	558·1	542·3	547·2
Textiles	408	428·3	419·7	421·8	435·6	429·0	423·4	415·1	403·0	396·3	391·3	385·7	384·7
Sundry	338	358·5	350·4	354·3	342·8	336·8	332·1	331·5	328·7	326·8	326·2	327·6	334·9

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TABLE 5

Average Prices of Each of the 45 Commodities

1953 to 1957

		Average Prices					*Index Numbers* (1866–77 = 100)				
		1953	1954	1955	1956	1957	1953	1954	1955	1956	1957
Vegetable Food											
1 Wheat, English Gazette	*s.* and *d.* per qr.	138 0	121 0	98 7	109 4	92 6	253	222	181	201	170
2 Manitoba	,, ,, ,,	112 5	124 7	127 6	130 9	122 0	200	222	227	233	218
3 Flour	*s.* per sack (280 lb.)	102⅓	95¼	97¼	103⅓	102 5/16	222	207	211	225	223
4 Barley, English Gazette	*s.* and *d.* per qr.	102 4	91 11	92 11	91 8	82 8	275	236	238	235	212
5 Oats, ,, ,,	,, ,, ,,	68 0	62 10	73 1	68 9	63 8	262	242	281	264	245
6 Maize, American mixed (*a*)	*s.* per qr.	122½	114 9/16	114⅜	119 5/12	100¾	376	353	352	368	310
7 Potatoes, good English†	,, ton	300	304	374⅝	454½	397½	257	260	320	388	340
8 Rice, Burma. No. 2	*s.* and *d.* per cwt.	107 9	90 0	174 3	63 6	63 9	1077	900	743	636	638
Animal Food											
9 Beef, prime	*d.* per 8 lb.	176	187	234	200	204	300	317	397	340	346
10 ,, middling	,, ,,	150	170	222	184	190	300	340	444	368	380
11 Mutton, prime	,, ,,	208	196½	214	205¼	201	330	312	340	326	320
12 ,, middling	,, ,,	113	139	182	181	191	206	253	331	330	347
13 Pork, large and small, average	,, ,,	202	196	206	218½	208	389	377	396	420	400
14 Bacon, Home Produced, Grade 1	*s.* per cwt.	339	296	270⅝	298	276½	458	400	366	403	374
15 Butter§	,, ,,	292½	346	364	339	294	234	277	291	271	235
Sugar, Coffee and Tea											
16 Sugar, Java floating cargoes (*b*)	,, ,,	26 5/6	26⅛	26	28¾	41	94	92	91	101	144
17 ,, Continental whites	,, ,,	36¼	35 3/16	37 1/16	42½	54¾	157	153	161	185	238
18 Coffee, Ceylon plantation, low middling (*c*)	,, ,,	448⅔	609	438½	491	439	516	700	504	564	505
Rio, good	,, ,,	387	481	312	321⅓	313	604	752	488	502	489
19 Tea, average import price	*d.* per lb.	40·03	54·65	64·61	53·62	54·21	234	310	375	311	314
Minerals											
20 Iron, Scottish pig	*s.* and *d.* per ton	328 9	344 8	372 4	428·2	464 6					
Cleveland (Middlesborough) pig	,, ,, ,,	274 10	296 3	324 7	350·5	401 0	468	500	540	603	671
21 bars, common	£ per ton	28⅜	31¼	38⅞	43⅜	45	344	379	471	526	545
22 Copper, Standard	,, ,,	254⅙	248⅞	351½	328¾	219⅝	340	332	468	438	293
23 Tin, Straits	,, ,,	721 7/16	707 15/16	740⅝	787 7/16	754¼	687	670	705	750	719
24 Lead, English pig	,, ,,	91 3/16	96⅝	106 1/16	116¼	96 5/8	447	472	520	567	471
25 Coal, Best Yorkshire House*	*s.* per ton	69½	62¼	66⅝	74¼	79¼	316	284	303	337	362
26 average export price	,, ,,	89·03	86·75	86·96	105·41	116·23	712	694	696	843	930

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TABLE 5—*cont.*

		Average Prices					Index Numbers (1866–77 = 100)				
		1953	1954	1955	1956	1957	1953	1954	1955	1956	1957
Textiles											
27 Cotton, Middling American	*d.* per lb.	31·83	32·72	31·70	26·95	25·35	354	363	352	299	282
28 East Indian	,, ,,	28·73	32·81	31·61	27·58	27·55	426	486	468	409	408
29 Flax, Medium grade continental retted	£ per ton	321½	318⅝	312½	282⅓	270⅔	628	635	592	543	511
Belgian import price	,, ,,	269·9	279·4	243·7	228·4	210·1					
30 Hemp, Manila for roping	,, ,,	125	92	92⅜	96	113	474	396	470	474	465
Italian S.B.	,, ,,	244¾	216¾	274⅛	273½	250					
31 Jute, L.J.A. Firsts	,, ,,	79¼	94	84⅝	89½	104¼	417	495	445	471	549
32 Wool, Merino, Port Philip, average fleece	*d.* per lb.	126¼	111¼	99⅜	102 7/12	100¾	665	579	493	546	550
Merino, Adelaide, average greasy	,, ,,	80¾	69⅛	54	67⅓	70 7/12					
33 English Lincoln half hogs	,, ,,	60	63¼	61¾	61⅞	68½	304	320	313	313	347
34 Silk, Japanese	*s.* per lb.	40	36⅔	34¼	32 7/12	33 1/12	174	160	149	142	148
Sundry Materials											
35 Hides, River Plate, dry‡	*d.* per lb.	22 5/16	19⅜	17 5/16	21¼	24	309	281	245	253	272
River Plate, salted	,, ,,	20¼	19	16 1/16	15¾	15⅞					
average import price	,, ,,	27·47	25·02	21·71	20·95	22·40					
36 Leather, dressing hides	,, ,,	57	55⅞	49¼	49	49	380	390	387	373	378
average import price	,, ,,	75¼	79¾	85¼	80½	82½					
Tallow, Town	*s.* per cwt.	66¾	68¼	73⅜	73⅜	74¾	148	152	163	163	166
Oil, palm	£ per ton	83	71¼	84⅛	97	92⅛	213	183	216	249	236
olive	,, ,,	305⅜	263 13/16	260	416	345	611	528	520	832	690
linseed	,, ,,	180	80¼	102⅝	137½	119⅛	443	292	345	411	341
Seeds, linseed	*s.* per qr.	209¼	182 5/16	207½	232½	188⅛					
41 Petroleum, motor spirit (c.i.f.)	*d.* per imp. gall.	12·32	12·08	11·53	11·81	12·96	77	76	73	74	82
Kerosene (burning oil) c.i.f.	,, ,, ,,	11·36	10·87	10·86	11·74	12·82	180	172	172	186	203
gas oil, c.i.f.	,, ,, ,,	10·76	10·32	10·70	11·44	13·03	270	259	269	288	327
42 Soda, crystals	*s.* per ton	190	190	212½	222½	222½	206	206	231	242	242
43 Sodium nitrate	*s.* per cwt.	29¾	27	27¼	28	30	212	193	195	200	214
44 Indigo, Bengal, good consuming	*s.* per lb.	12½	12½	12½	13¾	15	172	172	172	190	207
45 Timber, hewn, average import price	*s.* per load	374·64	338·12	338·44	342·64	340·62	758	717	762	777	768
sawn, or split, average import price	,, ,,	489·49	479·04	530·16	543·02	534·72					

(*a*) La Plata from 1924–40; Argentine (Feeding Stuffs) from 1941–52; Yellow American subsequently. (*b*) Raw centrifugals. 96 per cent. Pol., from 1924. (*c*) East India, good middling from 1908–1947; Kenya from 1948. * Owing to change in selling methods the quotation for house coal alone, no longer ascertainable; from 1954 the quotations relate to the average pit price of all grades of Yorkshire coal. † Average of weekly quotations during the eight months, January to April and September to December. ‡ Nigerian Butcher Dry from 1956; Dry Bahias not quoted. § Dutch butter to 1954; subsequently average of Australian, New Zealand and Danish.

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TABLE 6

Index of Silver Prices

The base of the Index Numbers given below is 60·84*d.* per standard ounce = 100, this being a parity of 1 fine oz. of gold to 15½ standard ozs. of silver.

	Price per oz. Standard (d.)		*Index Number*	
	Average for year	*At end of December*	*Year*	*At end of December*
1942	23½	23½	18·1	18·1
1943	23½	23½	18·1	18·1
1944	23½	23½	18·1	18·1
1945	30½	44	23·1	31·1
1946	48⅝	55½	36·7	41·6
1947	44 7/16	45	33·3	33·8
1948	45	42½	34·1	31·9
1949	49⅕	64	32·8	33·3
1950	64⅕	70	33·7	36·4
1951	77 27/32	77	40·5	40·1
1952	74⅜	72¾	38·7	37·9
1953	73 15/16	73¾	38·5	38·5
1954	73½	74¼	38·4	38·6
1955	77½	78¾	40·0	40·8
1956	79⅛	79½	40·8	40·9
1957	78 9/16	77⅛	40·7	40·0

TABLE 7

Gold and Silver: Production

	Gold	*Silver (million ounces)*				
	Value of output (£ *thousand*)	*United States*	*Mexico*	*Canada*	*Australia*	*Central and South America*
1942	289,153	54·5	84·9	21·8	14·2	31·8
1943	226,397	44·8	71·2	18·6	10·3	29·3
1944	209,031	37·4	63·0	14·8	9·4	29·7
1945	199,116	29·3	61·1	14·0	8·1	27·4
1946	202,435	21·4	48·3	13·6	9·0	26·1
1947	206,434	36·1	49·2	13·5	9·5	23·6
1948	214,647	36·1	45·8	17·0	10·1	23·0
1949	320,887	34·6	49·5	17·6	9·8	24·0
1950	329,580	42·0	49·0	23·2	10·7	28·0
1951	322,623	39·9	43·8	23·1	10·8	28·7
1952	329,381	39·8	50·4	25·2	11·4	32·6
1953	325,500	37·7	47·9	28·3	12·4	35·5
1954	344,865	35·6	39·9	30·7	13·8	33·2
1955	340,732	36·5	48·0	27·7	14·5	35·3
1956	351,600†	38·7	43·1	27·7	14·5	36·3
1957*	366,543†	37·5	46·0	24·2	14·5	37·0

* Estimate. † Excluding U.S.S.R.

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REVIEWS OF STATISTICAL AND ECONOMIC BOOKS

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1.—*Games and Decisions: Introduction and Critical Survey*. By R. Duncan Luce and Howard Raiffa. New York, Wiley, 1957. London, Chapman & Hall. xix, 509 pp. 9″. 70*s*.

Students of game theory are often asked to mention some practical applications. They must, more defiantly than despondently, admit that there is little than can be quoted. Yet usually a discussion of mutual benefit ensues, the trend of which depends on the specific interests of the enquirer: mathematician, psychologist, business man, operational research worker, statistician, political or social scientist.

The present book, dedicated to the memory of John von Neumann, is a study of the Behavioral Models Project, Bureau of Applied Social Research, Columbia University, and hence mainly addressed to the last-mentioned category. Its attitude is stated on p. 10 to be as follows: "Because there has not been a plethora of applications in a dozen years, it does not follow that the theory will not ultimately be vital in applied problems". This slightly apologetic mood is in keeping with the following paragraph: "This book may be correctly accused of having a critical tone. We do not, however, intend this to be carping. . . . Our aim is to warn and to challenge the reader at just those points where the theory is conceptually weak". (P. viii.) Your reviewer thinks that in this and in other aims the book is quite remarkably successful.

Its structure can best be described as being fashioned on the von Neumann-Morgenstern classic (which contains more, if rather elementary, mathematics) : utility theory, normal form of games, two person zero-sum and non-zero-sum games, *n*-person games, characteristic functions, solutions. Into this framework the authors have fitted nearly every more recent development, such as the Nash theory of non-cooperative games, decision making under uncertainty, arbitration, survival, and attrition games, stochastic games, psi-stability (a concept due to the first-named author), and much more. The last (14th) chapter "Group Decision Making" contains an account of Arrow's book *Social Choice and Individual Values* (Cowles Comm. Monograph no. 12, 1951).

Marked sections contain more advanced material and some give the gist of a mathematical argument in a way which need not scare off even the most anti-mathematical

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reader. "Neither the calculus nor matrix algebra as such are required, but neither will hinder, for probably the most important prerequisite is that ill-defined quality: mathematical sophistication." (P. viii.) The latter quality is perhaps mostly required in the eight appendices, which deal with a probabilistic theory of utility, the minimax theorem, geometrical interpretations, linear programming, solving games, and with sequential compounding of games, a rather recent development. A Bibliography (fairly complete in game theory) and an Index follow.

Of topics as yet controversial in statistical circles "statistical decision making" is dealt with in sections 13.7 ff., and section 13.10 is entitled "Classical statistical inference versus modern statistical decision theory: some very brief comments". They cover more than eight pages, but this is brief in a text of 484 pages in all.

This book seems to achieve the impossible, by being very readable (if too facetious in places), not too high-brow and not too pedestrian, grinding its axe almost imperceptibly, and being suitable as a comprehensive book of reference presumably for quite a long time to come. Paper and print are, of course, excellent, as they ought to be at their price.

S. VAJDA.

2.—*An Introduction to Probability Theory and Its Applications*, Vol. 1. By W. Feller, New York, J. Wiley, 1957. (London, Chapman and Hall.) 2nd ed. xv, 461 pp., 9″. 86*s*.

The first edition of this outstanding book appeared in 1950 and it is now firmly established both as an introductory text-book and as a reference work on more advanced topics. For the second edition Professor Feller has put in a new chapter, carried out extensive minor revision and added a considerable number of further exercises.

Chapters 2 and 3 of the first edition, on combinatorial analysis and on occupancy and ordering problems, have been combined, and the new Chapter 3 is called Fluctuations in Coin Tossing and Random Walks. In a series of mutually independent trials, values of $+1$ and -1 are assumed to be equally likely on each trial and S_m is the sum of the first m outcomes. A direct and elegant investigation is made of such questions as the distribution of the proportion of $S_1, \ldots, S_n$ that are positive, the distribution of the number of zero values among $S_1, \ldots, S_n$ and so on. The general conclusions are, roughly speaking, that it is quite likely that most of $S_1, \ldots, S_n$ will have the same sign, and that the number of zeros increases relatively slowly with n, in fact only as fast as $\sqrt{n}$. As in the remainder of the book, the theorems are vividly illustrated with numerical examples.

One may quibble at the statement on p. 142 that the weak law of large numbers is "of very limited interest and should be replaced by the more precise and more useful strong law". Certainly the strong law is of great interest conceptually and mathematically. It seems, however, of no direct practical relevance, whereas the weak law is appealed to implicitly whenever the statistical fluctuations in a large set of data are ignored, since in such a situation we are concerned with behaviour for a single large value of n.

Material in the first edition has been expanded to form a new Chapter 12 called Compound Distributions and Branching Processes. There is included a discussion of the discrete infinitely divisible laws. The new Chapter 13 is a combination of the two chapters in the first edition on recurrent events.

The treatment of Markov chains has been somewhat modified and in particular the terminology has been slightly changed. Let f_j be the probability that starting from state E_j the system returns to E_j at some future time. If $f_j = 1$ the state is called *persistent* instead of recurrent as in the first edition. The previous language clashed a bit with the general definition of a recurrent event.

The final chapter consists, as in the first edition, of an admirable account of stochastic processes with discrete states in continuous time. The only major change consists of an improvement in the discussion of processes with a finite chance of "escaping to infinity" in a finite time. A minor criticism may be made of a numerical example in an earlier part of the chapter (p. 420). This is concerned with the so-called machine interference problem with several repair men. Two situations are compared: (*a*) one operative in

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charge of six machines, (*b*) three operatives jointly in charge of 20 machines, each operative being free to repair any of the machines. From tables of C. Palm, numerical results are given for a particular rate of occurrence of stops and mean repair time. The results are expressed in terms of "coefficients of loss" where, for example, the coefficient of loss for machines is defined to be

$$\frac{\text{average number of machines in waiting line}}{\text{number of machines}}.$$

It is concluded that "three repairmen per twenty machines are ever so much more economical than one repairman per six machines". In fact, however, the rates of production per machine differ in the two cases by only about 3 per cent. The point is that the loss of production from interference is in any case rather small in the particular situation considered. The advantages of combined service are better shown at a higher loading.

D. R. Cox.

3.—*Introduction to Statistical Reasoning*. By Philip J. McCarthy. New York, Toronto and London, McGraw-Hill, 1957. xiii, 402 pp. 9″. 43*s*. 6*d*.

This is an unusual book in the rather crowded field of elementary statistical texts. It contains the standard type of material to be expected in this kind of book, but the mode of treatment and the thoroughness of the explanations provided are quite remarkable. Almost every fresh technique, however minor, is accompanied by one or more examples with a practical flavour, giving details of the computations required. Further interesting, and mostly fresh, exercises are given at the end of each chapter. Answers to the exercises are not provided.

This book should be useful to beginning students in statistics and particularly to students working on their own. Provision of answers to the exercises would improve matters, but the only other criticism would be on the grounds of possibly over-lengthy exposition. Some students may find that they waste time in elaborate repetition of already well-established points. But the author would probably reply, quite reasonably, that the book is intended for the other type of student.

The topics covered in the book include—terminology used in statistics, distributions treated numerically and graphically, measures of location and spread, random sampling and probability, binomial and Normal distributions, elementary sampling design, χ^2 procedures and linear regression (with a brief treatment of multiple linear regression). The Poisson distribution is not considered in the text, although it does occur in one exercise. General problems of statistical inference are introduced and discussed in connection with the binomial distribution. The discontinuity of this distribution makes it a rather unfortunate vehicle for a discussion of general problems of scientific inference. However, the definitions of confidence interval (and coefficient) and of the two types of error (false rejection and false acceptance) are reasonably clear, and repeated sufficiently to drive home their meaning.

No acquaintance with mathematics is assumed beyond the use of symbols to represent observed variables, and the operations of addition, subtraction, multiplication and division. The book is thus really elementary in regard to mathematical concepts but has successfully followed the principle (quoted from the dust-cover) that "The use of a non-mathematical approach does not absolve one from exercising great care in stating the reasoning and techniques involved in statistics".

The book is well produced but even so rather expensive for its size. Nevertheless for the relatively non-mathematically inclined it provides an excellent way of finding the way in which a good deal of statistical reasoning works.

N. L. Johnson.

4.—*Queues, Inventories and Maintenance*. By P. M. Morse. Publications in Operations Research, No. 1. New York, Wiley, 1958. London, Chapman and Hall. ix, 202 pp. 9″. 52*s*.

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Professor Morse has written an excellent introduction to the theory of queues, applying a single mathematical technique to a wide variety of systems. The general implications of each result are clearly discussed and the behaviour of the main formulae is illustrated graphically; a further volume is promised with extensive tables, and there is mention also of a possible third volume on maintenance theory. The central ideas of input, queue discipline, etc., are illustrated by brief, rather idealized, examples, but there is no attempt to discuss real applications in detail.

The mathematical technique used is that of constructing a system of simultaneous linear equations satisfied by the equilibrium probabilities. It is well known that this can always be done when all distributions associated with the system are exponential. Distributions with coefficient of variation less than one are represented by χ^2 distributions with even numbers of degrees of freedom, for which Erlang's device of a series of exponentially distributed stages can be used. Distributions with coefficient of variation greater than one are represented by exponential distributions in parallel. No other distributions are considered in the book.

It follows that much recent work on queueing theory is not dealt with. In fact, the author's aim, as set out in his Preface, is not to give a comprehensive account of the subject, but rather to present a simple technique which can be used by the operational research worker to set up and solve his own special problems. The choice of subject matter, the clarity, and the mathematical level of the book, should combine to make it very valuable to workers concerned with applications.

Some idea of the subjects treated can be gained from the chapter titles: Representation in Terms of Probabilities; Probabilistic Description of Arrivals and Service Times; Single Exponential Channel; Multiple Exponential Channels; Simulation of Non-Exponential Distributions; General Considerations; Transient Solutions; Single Channel, Infinite Queues; Multiple Channels, Infinite Queues; Queue Discipline and Priorities; Problems of Inventory Control; Maintenance of Equipment.

The following brief remarks concern two general issues, sometimes of practical importance, not mentioned in the book. First, there is the question of the practical implications of the assumption that the service-times of different customers are independent random variables. It is easy to get the idea that to find the distribution of service-time for substitution in the theory, it is enough to measure a large number of service-times and to form the data into a frequency distribution. This will often do, but if, for example, much of the variation is accounted for by a slow trend in mean service-time, misleading results are likely, especially if the traffic-intensity is high. Similar remarks apply to the distribution of arrival times. In fact, it may be argued that, except in machine maintenance problems and in the queue discipline "strict rotation of customers among k servers", it is unlikely that the intervals between successive arrivals will be independent random variables, except in the random and regular cases. It may be that lack of independence is unimportant in practical situations, but this has not been shown.

The second general point concerns the idea of service availability. Restriction on service is the essence of a queueing system and can be of two principle types, a restriction on capacity (number of customers that can be dealt with simultaneously) and a restriction on availability (ability to deal with customers at all). In many practical cases, both types of restriction are present, for example the server in a single-server queue may have duties other than the service of customers. All the restrictions considered in the book are on capacity, although restrictions on availability are considered implicitly in the excellent chapter on queue discipline, since some restrictions on availability can be considered as arising from the service of a second, possibly fictitious, type of customer. Some explicit consideration of problems of availability would have been helpful. D. R. Cox.

5.—*Combination of Observations*. By W. M. Smart, M.A., D.Sc. Cambridge University Press, 1958. xiv, 253 pp. $8\frac{1}{2}''$. 35*s*.

The most obvious criticism of this book is that it is "old-fashioned" in its methods. Indeed, as the title suggests, the book contains similar material to Whittaker and Robinson's

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Calculus of Observations and earlier text-books, and there is very little deviation from tradition. However, the book also possesses the "old-fashioned" virtues of accuracy and thoroughness (within natural limits) although this does not extend to the provision of exercises to be worked by the students.

Among the features which may appear unusual to present-day readers (or, at any rate, statisticians) are: the derivations of the Normal law from Gauss' and from Herschel's assumptions; the use of the probable error (though the more familiar standard error appears frequently); Gauss' notation $[x\,y]$ for sums of squares and products; and the frequent use of "precision" (equal to $(\sqrt{2} \times \text{standard deviation})^{-1}$).

While the book will probably be found quite adequate by the students for whom it is intended, it appears, to an outside observer, to be a clearly presented but rather formal collection of rules of procedure which have been propounded by more or less authoritative scientists in the past, with but meagre provision for the development of independent judgment.

The subjects covered are: frequency distributions; least squares; probability and the Normal law; measures of precision; weighted observations; equations of condition (a long (45-page) chapter, mainly on various aspects of multiple regression); Gram-Charlier series and the Pearson system of curves; correction of observed statistics (an interesting chapter); and a relatively brief chapter on correlation, containing a section on the Normal trivariate surface with rather heavy mathematics which might have been reduced by first considering the general multivariate distribution. There is a table of erf (t) and short tables of $\exp(-k^2 x^2)$ and a function $G(q)$ used in "improving" the value of the magnitude of an observed vector.

N. L. JOHNSON.

6.—*Experimental Design in Psychology and the Medical Sciences.* By A. E. Maxwell. London, Methuen, 1958. 147 pp. $8\frac{1}{2}''$. 17*s.* 6*d.*

Intended for research workers and clinicians who have realized the need for a statistical approach, this book sets out to give some guidance in the choice of experimental design and in the appropriate analysis and interpretation of the results. It seems almost impossible to discuss design in a general way without becoming obscure to someone new to the terminology; the author has therefore resorted to the method of describing a few actual experiments in detail. The choice of examples in a work of this nature is to some extent a matter of taste, and will depend on the experience of the statistician concerned. The author, who works at the Institute of Psychiatry (University of London), deals only with objective psychological tests, mainly in the field of psychiatry—other branches of medicine are not discussed.

The presentation is clear, and this book certainly helps to fill a gap in statistical literature. Unfortunately it is impossible to recommend it without reservations, partly on account of certain weaknesses of design and interpretation, and partly on account of several errors in the chapter on analysis of covariance, which render it very misleading.

As the author points out, many research workers (especially in psychology), in attempting to study statistical methods, find themselves bogged down in the calculation of descriptive statistics. He overcomes this difficulty in an ingenious first chapter, in which he discusses basic ideas, such as the normal distribution, degrees of freedom, sampling and tests of significance. The next five chapters deal with Randomized Blocks, Latin and Graeco-Latin Squares, Factorial designs, Cross-Over designs, and Balanced Incomplete Blocks. The rationale of each design is discussed, and the analysis is illustrated with examples. In the examples given, however, the choice of design does not always seem entirely appropriate.

For example, a design employing two Graeco-Latin squares is used to test the effect of four drugs on an individual's sensitivity to light, at four different intensities; eight subjects are tested just once on each of four days. Thus it is not possible to test the various interactions, which must surely be important in this type of experiment. Very often in psychological experiments there are not the same limitations with regard to the size of the experiment as arise in other fields, and advantage can well be taken of this to have a simple

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design in which the interactions can be tested (particularly people × treatment) and more general conclusions can be drawn, than a complex design which obscures the picture. An example of an incorrect conclusion being drawn is to be found in the same chapter. A boy is discovered to have significantly greater difficulty in remembering digits presented visually rather than verbally, and the conclusion is that: "there is some dysfunction in the visual modality. . . . ". It has surely first to be shown that "normal" subjects do not behave in the same way.

After a brief (and clear) chapter on regression and correlation, in which the author confines himself to the bivariate normal distribution, he leads on to a chapter on the analysis of covariance. This is discussed in terms of an example in which the intelligence (X) and speed scores (Y) are given for three diagnostic groups. Since the means of both variables differ significantly between groups we are far from the model of the previous chapter, and it is incorrect to test the overall correlation, and to fit one line for prediction. The accompanying discussion is confused and inaccurate, and in any case this example is unsuitable to illustrate the use of analysis of covariance as the situation is a complicated one. In another example an erroneous attempt is made to correlate two *different* Latin squares (used in an experiment discussed earlier). Fortunately there is no attempt at a detailed interpretation of the result. A correct use of covariance analysis is, however, given in the last chapter, in a discussion of methods of increasing efficiency.

The following two chapters introduce two systematic designs, fairly recently developed by D. R. Cox, first, those in which only certain orders of treatment are admissible (this may arise, for example, in testing the effect of a drug, or of leucotomy); secondly, those which are designed so that linear (or higher degree) trends may be easily eliminated. A rather artificial example of the latter is given, in which it is not obvious that the basic assumptions are fulfilled. It is valuable that attention should be drawn to these designs, although one wonders if they are safe in the hands of the non-expert. A last brief chapter shows how to measure the relative efficiency of different designs. This is clear and useful, and some of the ideas could well have been introduced earlier; for example, the completely randomized design is first mentioned here.

Each chapter is followed by a list of references and books for further reading. The general arrangement of the book is good, and the method of each analysis is clearly shown, so that, at least for the simpler designs, an experimenter should have little difficulty in adapting them to his purposes. Several of the tiresome problems that arise in psychological and clinical experiments, such as in repeated testing, are discussed lucidly, and the need for randomization is stressed. However, it is a little disappointing that not more attention has been paid to some of the other snags that arise in practice, such as the selection of the appropriate error term; a comment on the choice of design in relation to the population about which an inference is to be made would also have been welcome.

Many research workers in this field may obtain useful hints for designing and analysing their experiments from this work; but there are so many pitfalls in every experiment—not all of which have been avoided here—that this book cannot, I think, be regarded as an adequate substitute for consultation with a competent statistician. M. A. CREASY.

7.—*An Introduction to Genetic Statistics.* By Oscar Kempthorne. New York, Wiley, 1957. London, Chapman and Hall. xvii, 545 pp. 9″. $12.75. 102*s*.

This is the first book covering in one volume all important topics in genetical statistics. It begins with an introduction to probability theory and its application to simple genetical segregations. It goes on to deal with applications to populations, both in the wild and subjected to special systems of breeding in the laboratory. After this, significance and estimation are discussed, and then the analysis of variance, regression, and correlation between relatives. The book ends with more advanced population genetics, including questions of selection and assortative mating.

This is a very good book. One can disagree with the author on points of detail and balance: for example, the section dealing with human genetics is woefully short, and deals

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mainly with a special and rather theoretical problem, that of the effect of methods of ascertainment on segregation ratios. (But some other problems in human genetics are discussed elsewhere in the book.) Also, generally speaking, theoretical principles receive more attention than practical methods of statistical analysis. However, the author has done remarkably well to cover so many aspects of the subject in one book, and has added a well chosen bibliography for further reading, so that the student will easily find out about methods which cannot be described in the book for lack of space.

It is clear that Prof. Kempthorne has taken an immense amount of trouble to put the material in a connected and logical form, and has himself filled in some gaps in the known results to achieve this. The book is very clearly written, and very readable. The mathematics required to read it extends only up to simple differential calculus and an understanding of the summation notation; matrices, etc., are defined in the text when required, and where a rigorous proof would require more advanced mathematics, this fact is clearly stated and an intuitive justification given instead where possible.

In short, this book is indispensable for all, both geneticists and statisticians, who wish to know something of the application of statistics to genetics. It is unfortunate that the price is so high, but that is the fate of almost all text-books at present.

CEDRIC A. B. SMITH.

8.—*Statistical Exercises, Part II.* Compiled by N. L. Johnson. London, University College, 1957. iv, 107 pp. $10\frac{1}{2}''$. 12*s*.

It is a platitude that the student of statistics cannot become a fully-fledged statistician without carrying out a good deal of practical work, and at first sight it would seem that there would be no shortage of exercises to meet the student's need. Every statistician must have plenty of material accumulated over the years and waiting to be used for such a purpose. It is only when one comes to teach a class in practical statistics that one realizes that this accumulated material does not satisfy all the requirements. In the first place there are some elementary statistical techniques, that the student needs to know, that one seems never to have had the occasion to use, and in the second place some of the material that, at first thought seems suitable, turns out not to be, for one reason or another, when the data are actually unearthed. It is then that books of exercises are turned to with gratitude.

Part I of this series—Elementary Statistical Exercises, compiled by F. N. David—appeared in 1952, and covered use of machines and tables; frequency distributions and their moments and other measures; the normal curve; correlation and regression; the binomial, negative binomial and Poisson distributions; tests for randomness; 2×2 tables; applications of χ^2; significance tests, power curves, and confidence limits; and simple quality control.

Part II, now available, covers in some detail all the standard analysis of variance techniques—simple groups, subclassification, cross-classification, Latin squares, factorial experiments, confounding, fractional replication, split plots, etc.—regression analysis, including multiple and curvilinear regression; dosage-mortality techniques; discriminant functions; time series; and the fitting of frequency curves.

The examples on each technique have a short explanatory text to go with them, but these sections do not, and are not intended to, give full directions for the calculations. It is assumed that the student will be studying a text-book in addition. The only exercise for which full instructions and the complete answer are given is the Choleski method of inverting a symmetric matrix, although it would have been helpful here to have included a word on "scaling" a matrix (to make all the elements of roughly the same order) before plunging into the arithmetic of inversion. This is not necessary for the matrix given in the exercise, but forms an uncomfortable trap for someone who has done the exercise and thinks that he can now deal with any symmetric matrix.

The reproduction is in typescript, but the result is just as clear as a printed book (which is by no means always true of typescript mathematics) and the binding is good.

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Past students of University College Department of Statistics will recognize a number of the exercises, but by no means all of them. All told this is a most useful publication, which deserves a recommendation. I. D. HILL.

9.—*Survey Methods in Social Investigation.* By C. A. Moser. London, Heinemann, 1958. xiii, 352 pp. $8\frac{1}{2}''$. 35*s*.

This book is based on lectures given by the author on survey methodology to students in the social sciences at the London School of Economics. It is aimed primarily at this class of reader and gives an admirable introduction to the subject. The student will appreciate the absence of that rash of Σ's which must so often frighten the non-mathematical reader of text-books on this subject.

The book deals with the history of surveys in Britain, the basic ideas behind different types of sample design, the different methods of collecting information, with interviewing, the design of questionnaires, response errors, and the processing and analysis of the results. Although the author recognizes that sampling is often the least of the problems facing the research worker, he nevertheless devotes over one-third of the text to this subject, since he finds that this is the aspect of surveys which is most difficult for the non-specialist to grasp.

The discussion on the principles of sampling is followed by a step-by-step calculation of the sampling error for an attribute taken from a national sample inquiry on which the author acted as consultant. In presenting a real-life example the author has skated over one or two points that may puzzle and mislead the student. The sample is one of adults aged 16 and over. It was obtained by first selecting a sample of electors, taking every n^{th} name from the electoral register. The interviewer had then to locate the household of which the chosen elector formed a part, or, if the elector had moved, the household which had moved in. All the persons aged 16 and over in the household were then listed, males first and then females, in descending age order. Which of these to interview was settled by reference to a table and depended on the serial number of the address and the number of persons listed. To obtain a correctly weighted sample of individuals the following weight was applied to each interview:

$$\frac{\text{No. of adults aged 16 and over in household}}{\text{No. of adults aged 21 and over in household}}$$

Where there has been no movement and there is only one household at the address, the denominator of the fraction will equal the registered electors at the address. Otherwise the denominator has to be obtained at the interview. The reader however is left with the impression that this could be obtained from the electoral register. The register does not tell one how many households there are at any address and who belongs to which household.

The re-weighting was accomplished by rejecting or duplicating punched cards: 900 out of 3,117 were affected, the final number of cards being 3,243. In comparing his estimated sampling error for the multi-stage sample design with that for a single stage random sample, the author takes the sample size as the re-weighted figure of 3,243, which is a little unfair to the multi-stage design. It might also lead the student to suppose that such re-weighting of a single stage sample could be ignored.

At the end of the worked example the author stresses, and rightly so, the misleading estimate of error which would have been obtained if the calculation had been made as for a simple random sample. However, he is again less than fair to his multi-stage sample, for he has not stressed that this estimate is in fact a considerable over-estimate. It arises because the systematic selection within strata of administrative districts listed in descending order of industrialization index has introduced an appreciable, and in the case of car ownership effective, degree of stratification which has to be ignored in the calculation. That there might be a negative correlation between industrialization index and car owner-

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ship is suggested by the three towns of one stratum given to illustrate the method of calculation. For these three towns

$I_1 = 044$ Newbiggin $p_1 = 4/50 = 0{\cdot}080$
$I_2 = 027$ Tynemouth $p_2 = 8/47 = 0{\cdot}170$
$I_3 = 004$ Windermere $p_3 = 11/54 = 0{\cdot}204$

where p gives the proportion in the sample saying they owned a car and I is the industrialization index for the town. The existence of a negative correlation is confirmed by other data. Neglecting this stratification leads to an estimate of error greater than would have been obtained if the towns had been selected at random. The sample has gained in precision but the error estimate has not.

This chapter has been dwelt upon at some length as it is suspected that it will be well thumbed by the student.

On the conduct of pilot inquiries there is one important piece of advice which the author should have given. While the pilot fieldwork is in progress the researcher should go out and observe half a dozen interviews made by each of half a dozen interviewers, not the best ones. This is time-consuming but very profitable. The field workers' reports which the author considers so useful are not in the reviewer's experience nearly as helpful nor a substitute for this.

On the results obtained from postal questionnaires addressed to the general population, the author is rather out of date, but this is through no fault of his since the results have not been published. Recent work at the Social Survey has shown that very good response rates can be obtained from the general population on a variety of subjects if simple short questionnaires are used.

The book seems to be largely free from printing errors, but perhaps one should be mentioned. In Table 8.7, which will be referred to frequently, the fourth column should be headed $\sum_{i=1}^{r} (p_i - p)^2$.

Notwithstanding these criticisms the book contains much useful instruction and advice, and can be thoroughly recommended for the student. Practitioners, too, might spare a little time for this readable book.

P. G. GRAY.

10.—*Economic Models: An Exposition.* By E. F. Beach. New York, Wiley, 1957. London, Chapman and Hall. xi, 227 pp. 9″. 60*s*.

Over recent years economists have tended to call to their aid in increasing degree the tool of mathematics, often in the form of a model, and this development has caused difficulties both to those trained economists who lack facility in the use of the tool and to those students of economics who have abandoned the study of mathematics some years earlier. The perfectionist solution to this problem, that those so handicapped should embark on a full course in mathematics, may be appropriate in a few special cases but, clearly, it is not reasonable for the vast majority.

This book attempts to assist both of the groups mentioned. It does not seek to add to current knowledge of mathematical economics or statistics but, rather, to explain in readily digestible fashion, some of the principal ways in which an economist may use mathematical and statistical techniques. To this end the book is divided into two parts, the first dealing with mathematical models and the second with econometric models.

The mathematical model is merely a set of relations between a number of economic variables, of which the best known is probably the supply/demand relationship. Accordingly, the introductory chapters lead into the analysis of the simplest relationship of all where both the supply function and the demand function are linear functions of the price, from which it is but a short step to a more complicated system of non-linear relationships—such as those representing the Keynesian model. The remainder of the first part of the book is devoted to problems involving time, both the continuously varying function (such

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as a price function) and the discrete function (such as annual national income figures) being treated.

The second part of the book treats econometric problems and, necessarily, these chapters are more difficult; indeed it would not be possible to develop the ideas in these chapters with the simplicity of the earlier chapters. Only loosely can these chapters be regarded as treating economic models, since the whole emphasis is on statistical theory; the author attempts to compress into less than 100 pages a summary of statistical theory, starting with the definitions of "probability", and proceeding by way of small sample theory and tests of significance to regression analysis, autocorrelation and multiple relations.

It seems to the reviewer that the book fails in at least two respects, firstly, that it tries to cover so much ground that few, if any, will find more than one or two chapters helpful and, secondly, that it tends to imply that the problems are mathematical. For the purpose of model-building, the mathematical approach is primarily an alternative system of communication of ideas—a shorthand which has the advantage of considerable clarity. If one is attempting to instruct non-mathematicians in this "language", it is surely desirable neither to suggest that algebraic manipulation of equations is the core of the matter, nor to imply by emphasis on statistics that realistic deductions may follow attempts to put values on the parameters of the models. In short, it is possible that a non-mathematician, armed with this book, might slog, mechanically and without deep understanding, a little further through a paper or book describing a new model devised by someone else; it is very unlikely that he would tend to think and examine problems in mathematical terms himself.

H. C. COTTRELL.

11.—*Economic Concentration and the Monopoly Problem.* By Edward S. Mason. Harvard University Press, 1957. London, Oxford University Press. xvi, 411 pp. 8½". 48*s*.

The first volume in the Harvard Economic Series, published fifty years ago, was a history of the internal grants of monopoly in England during the sixteenth and seventeenth centuries. This centenary volume can also claim to make a contribution to economic history; it consists of Professor Mason's reflections over the last twenty years on the problems of monopoly and the large firm in the United States. In bringing together a series of papers written for different readers at different times, inevitably there is a lack of cohesion and some of the ground gets trodden more than once. In spite of these irritations, some interesting points emerge.

Professor Mason contends that economic concentration is not on the increase in the United States; indeed he thinks that probably American industry is no more or less monopolistic than it was fifty years ago. And if comparison is made with Leak and Maizels' study of concentration in British industry, the share of economic activity accounted for by the largest firms seems to be greater, industry by industry, in Britain than in the United States.

Professor Mason does not produce any new measures of concentration, but discusses —although not extensively—the studies of other research workers. No sophisticated statistical analysis or criticism appears in this book, but the paucity of sound economic information is only too evident. Professor Mason argues that in any case concentration ratios can be a dangerous tool from which conclusions may be drawn too hastily. To him they are the beginning and not the end of any study of monopoly.

The economic problem, as the author sees it, is to explain differences in competitive practices—embracing price, production, and investment policies—through an examination of the structure of markets and the organization of individual firms. But even a cursory examination of specific industries seems to destroy any hope of generalizations. "With respect to relative prices, costs and profits, we can make almost no universally valid statements about their relation to economic stability. . . . not only is our theory incomplete, but also the data on which to base a conclusive policy statement is virtually never available in time."

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Many theories of the 1930's about monopoly have gone out of fashion, leaving, however, just as many open questions. The usefulness of this book to the economic statistician is that it surveys the problems and the state of our ignorance. MARJORIE DEANE.

12.—*Time in Economics.* By G. L. S. Shackle. Amsterdam, North-Holland Publishing Co., 1958. 111 pp. $7\frac{1}{2}''$. *g.* 6.50. (Prof. Dr. F. de Vries Lectures, 2.)

It is always refreshing to read Professor Shackle's discussions of economic theory and it must have been not less rewarding to hear these three lectures delivered. While Professor Shackle is always clear on points of detail and his argument flows smoothly and freely, he is sometimes not so helpful in indicating where the broad sweep of his discussion has originated and where it has left us at the end. In the present volume Professor Shackle has portrayed his well-known theory about decision-making in economics, which he discusses in summary form in the second lecture, against the background of his personal views about the proper role of economic theory especially when it is concerned with situations developing over time. While he has much to say which is important and revealing, there are also some observations which seem indicative of a somewhat restricted point of view.

His first lecture is, broadly speaking, a plea or a claim that the individial has a distinctive part to play in economics, which is too often assumed away in the desire to build mechanical models of the trade cycle or of the growing economy. The making of decisions is a creative activity and as such must contain an irreducible element of unpredictability. A decision maker can only operate in one instant of time and enjoys future and past events at one remove by forming his expectations or summoning back his memories. Because we cannot build a model which allows an individual to exercise his creative powers as one decision follows another, economic dynamics must either be confined to individuals endowed with given expectations, or with large aggregates of people, or to periods in which institutional rigidities prevent creative activity from having a noticeable effect.

It is easy to sympathize with Professor Shackle's antipathy to models of the economy that appear to evolve or oscillate with machine-like precision. But while we should never forget that each man has a mind, surely we should learn, and have learned from the engineers, important things about the sources of instability, whether they emanate from ingrained habits of behaviour or from the mode of operation of existing economic institutions. We should be reluctant to accept his view that individual decision-making behaviour must be wholly unpredictable, unless expectations are already known. Professor Shackle's difficulty appears to arise from his inability to distinguish between different degrees of predictability and this in turn seems to be related to his reluctance to make use of or to appreciate probabilistic or statistical concepts. Surely there is room for theories which explain only a part of the observed regularities in individual behaviour and which recognize that there will always remain an inevitable and often large area of inexplicable irregularity?

This is not the place to discuss in detail Professor Shackle's second lecture, since his longer works on the subject of expectations are well known and nothing new is added here. The concept of potential surprise is an elegant and rewarding idea, and it may well be that many of us do think in these terms about future events. However, Professor Shackle's criticisms of orthodox probability concepts are not well founded. In brief, they rest on the misleading contention that a probability has no meaning or relevance unless the experiment to which it refers can be repeated a large number of times.

In his third lecture, Professor Shackle discusses simply and elegantly three theoretical problems. He explains why certain types of investment schemes gather momentum as they progress, how the investment multiplier may be used to generate a perpetual cycle, somewhat on the lines of the most simple versions of R. M. Goodwin's theory, and why the rate of interest is inherently restless. This selection of topics shows Professor Shackle at his best. With the most rudimentary tools and a clear insight he shows us the essentials of a mechanism without tying down individuals to anything but the most plausible behaviour pattern.

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Perhaps this review should end by asking Professor Shackle a question. If he believes that individual decision-making behaviour is inherently quite unpredictable, on what basis are we to decide whether his theories or rival ones are the most plausible?

A. D. Roy.

13.—*Economic Backwardness and Economic Growth.* By Harvey Leibenstein. New York, Wiley, 1957 (London, Chapman and Hall). xiv, 295 pp. 9″. 54*s*.

Why do some countries remain under-developed while others expand rapidly? Economists have addressed themselves in recent years to a study of the conditions for progress, and indeed an earlier work on this theme by the same author, *A Theory of Economic-Demographic Development*, was reviewed in this *Journal*, 1955 (p. 367). Now Professor Leibenstein, perhaps influenced by current ballistic theory, has developed the notion of "critical minimum effort"—an impetus that must be of a certain size before an economy can emerge from poverty, just as a rocket needs a boost to a certain velocity in order to escape from the earth's gravitational field.

In economics one cannot expect the precision of mathematics, or even the hesitant success of rocketry, and the author firmly dissociates himself from factual measurements and details (which would in any event be inadequate) in order to clear the way completely for investigation in the abstract—"the type of 'free-wheeling' speculation in which there is neither a commitment to, nor any great concern with, the practical aspects of the problem". In spite of its almost completely non-statistical nature, this book contains many interesting ideas that may well have an important influence upon economic thought in future.

The text falls loosely into four parts: an introduction, in which the *modus operandi* is outlined; an analysis of economic backwardness and the reasons for stagnation; a consideration of the factors conducive to escape from backwardness; and finally a critique of the views of other writers on the way to ensure rapid development.

The author takes *per capita* output as his index of development and regards as "quasi-stable" an economy in which increasing production is so matched by increasing population that this index remains constant; (population is regarded as a wholly dependent variable, and so if any change occurs in fertility or mortality it is solely for an economic reason; its part in the analysis is not a very prominent one). He gives a comprehensive list of the characteristics of "backwardness" and discusses in detail the mechanism of the vicious circles in which many economies revolve. Only questions of foreign trade and of monetary mechanism are omitted from the analysis.

Although it is difficult to envisage how a country can break away from under-development, the knowledge that some have done so is a spur towards theory-formation. Professor Leibenstein, pointing out that there is in practice an upper limit of some three per cent. per annum to the rate of population growth, considers that if for a few years the production rate can be raised well above this level a chain reaction will be started (including a decline in fertility) that will ensure further progress thereafter. This part of the analysis is perhaps less convincing than his earlier pages. One of the difficulties to which free-wheeling speculation may pay insufficient attention is the likelihood, noted by Bauer and Yamey in their book, *The Economics of Under-developed Countries* (1957, C.U.P.) that the contemporary existence of highly-developed economies may retard capital formation in the underdeveloped countries because a strong public appetite for increased immediate consumption may be created which tends to bite deeply into any increased production and leave relatively little over for investment in long-term growth.

One of the advantages of the author's framework of ideas is that it gives him a background against which he can assess the worth of other economists' investment policies. His study of the effect of an increased investment allocation upon the growth of skills and of "entrepreneurship" is of particular interest in this connexion, difficult though these may be to measure in practice.

P. R. Cox.

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14.—*Measurement of Levels of Health* . . . World Health Organization, Technical Report Series, No. 137. Geneva, 1957. 29 pp. 9½″. 1*s.* 9*d.*

The two objects of this report were (1) a review of the existing methods of measuring health and suggestions for their improvement and (2) suggestions for new methods. For a community the concept of health is vague and health is a relative term which has been measured by many indirect methods. The Study Group responsible for this report have discussed some indices in detail but have not made any positive suggestions, probably, because they were unable to define the problem they were exploring. Their review of the existing statistics that can be used for an indirect measure of health could be expanded. The use of the expectation of life is dismissed quite shortly. The limitations of the crude death rate for international comparison is stressed but no mention is made of the fact that within a country the crude death is not a reliable measure over relatively short periods. During the part thirty years the crude death rate in England and Wales has fluctuated around a fairly constant level but it is possible to find crude death rates in recent years that are slightly above the rates of twenty years earlier although the death rates in each age group are smaller, the fall being over 50 per cent. in a number of age groups. The infant mortality is stated to be traditionally of great significance as an indicator of health. While infant mortality is generally correlated with the general death rate vital statisticians have generally used it as an indirect index of social, economic, hygienic conditions. A proportional mortality ratio, deaths at ages 50 and over as a percentage of total deaths, was considered and the Group recommended that further studies of this ratio with varying limiting ages other than 50 should be explored. They have not pointed out that international comparisons must be affected by the size of the birth rate; an increasing and static population with the same age specific death rates would have quite different ratios. Most communicable diseases are preventable and the group suggested that a high death rate may reflect low levels of living. The use of death rates does not seem to be appropriate since the increase in medical science has reduced the mortality but the incidence may be unaffected. In England and Wales the large decrease in childhood mortality has been mainly due to the reduction in the mortality from infectious diseases but in many cases the disease is as prevalent as ever, e.g. measles. Again the death rate from dysentery is insignificant but the incidence has increased rapidly in the last few years.

The suggestions for new indices include further research in (*a*) morbidity, (*b*) nutrition, (*c*) mental health, (*d*) environmental sanitation, (*e*) health services, and (*f*) socio-economic conditions. The Group considered multi-purpose surveys of the type that has been carried out in America. They considered that information could be collected by individuals not professionally trained and suggest "Certain procedures such as taking blood specimens could be taught to any intelligent person in a relatively short time, as could the performance of such operations as taking blood pressure, doing simple tests such as tuberculin and Schick tests, and a number of others". It seems unlikely that any Western country would allow unqualified persons to do these duties. The success of any survey depends upon the co-operation of the population and it is quite unlikely that any population would co-operate in such a survey as outlined. Those persons who volunteer to co-operate could not be taken as representative of the general public.

This report contributes little to our knowledge and suffers from the defect that what is ideal and what is possible has not been clearly considered. In some of the schemes the practical difficulties are such that either the scheme is unworkable or it would be prohibitively expensive.

W. J. MARTIN.

15.—*The Survey of Sickness 1943 to 1952.* By W. P. D. Logan and Eileen M. Brooke. G.R.O. Studies on Medical and Population Subjects, No. 12. London, H.M.S.O., 1957. v, 80 pp. 9¾″. 4*s.*

This is the concluding report on an enquiry which began as a social survey in 1944, the original purpose of which was to estimate the amount and nature of illness amongst

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civilians over the age of 16 during war-time. It was essentially a questionnaire enquiry, based on a stratified sample of the adult population of England and Wales.

Commencing in January 1944, 3,000–4,000 persons were interviewed per month (each monthly sample representing approximately one person per thousand population) and the illnesses they said they had suffered from in the previous three months were recorded. From July 1949 the questions were restricted to the two months preceding interview.

The sampling procedure used is clearly recounted in the present report and was also discussed in a paper by Gray and Corlett, in this *Journal*, 1950.

The interviews were carried out by trained personnel of the Central Office of Information during the war and post-war years until the enquiry was suspended for reasons of national economy in 1952. Following a preliminary report (Slater, *Survey of Sickness, October 1943 to December 1945*. The Social Survey, 1946) the analysis of the material collected was carried out by the General Register Office and articles embodying the results have appeared at regular intervals in the monthly bulletins of the Ministry of Health, the annual reports of the Chief Medical Officer and more recently in the Registrar General's Statistical Reviews (Supplements on *General Morbidity*, 1949, and 1950–51). But the most familiar and widely quoted of all reports on this survey has been that of Dr. Percy Stocks (*Sickness in the Population of England and Wales in 1944–47*; Studies on Medical and Population Subjects No. 2). Indeed, it is fitting to remember that modern indices of morbidity—inception and prevalence rates (spells or persons), the point prevalence rate and medical consultation rates—which recently received the blessing of one of the Registrar General's Advisory Committees in an official publication (*Measurement of Morbidity—Studies on Medical and Population Subjects*, No. 8) were, under slightly different names, fathered by Stocks in that earlier report.

The essential feature of the survey was that the illness mentioned by the subject to the interviewer was recorded, i.e. no attempt to affix a medically certified diagnosis to the illness was made. This is the basis of one of the two major criticisms of those who doubt the validity of the survey—that people rarely know from what illnesses they are suffering and the allocation of such to diagnostic groups of diseases is artificial. To answer this, one needs to recall the circumstances in which the survey was initiated, and its original purpose. In 1943–44 there was a very strong impression that the health of the civilian population had deteriorated, particularly that the prevalence of minor ailments had increased owing to the strain of longer hours of work and additional civil defence duties.

At that time the only information on sickness outside the field of notifiable infectious diseases was that adduced from various studies during the previous 20 years on a few select groups. There was no source from which the national experience could be examined, and the importance of the preliminary reports on this survey is that they not only provided *some* information where none previously existed but also stimulated research from which other sources of information have subsequently been explored and developed. Yet there is still one aspect of morbidity on which this survey throws light but which all its derivatives ignore. Only 23 per cent. of persons who recorded an illness had consulted a doctor, so that the volume of illnesses experienced by the other 77 per cent.—admittedly of minor type, but accounting in the aggregate for much uncertified sickness absenteeism—is not represented in the more recent surveys of General Practitioners' records, hospital in-patient statistics, national insurance claims to sickness benefits, cancer registration, or statistics of mental health. Here, and here alone, is there any estimate of the *total* volume of ill-health experienced by the population.

Furthermore, for some diseases, whose prevalence could be estimated from other sources, the rates obtained in this survey were in sufficient agreement to indicate that the diagnostic classification was not entirely imperfect.

The other major criticism to which the survey has been subjected is that the vagueness of people's memories of their illnesses over the past two months is a source of error. But following a detailed analysis of the effects of the memory factor, the authors conclude that no drastic distortion of the principal statistical results was introduced.

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The detailed tabulations are hardly suitable for abstract and should be consulted in the original. Two illustrations must suffice:

(*a*) Age-group by age-group, women had higher monthly prevalence rates from all causes of illness combined (both spells and persons) than did men, and in most years their consultation rates were higher.

(*b*) Excluding a large residual group of ill-defined symptoms, the four diagnostic groups with the highest monthly prevalence rates per 10,000 persons interviewed were: Upper respiratory system—including influenza and the common cold (males 1,511, females 1,519), the rheumatic diseases (males 1,289, females 2,229), lower respiratory diseases—including bronchitis and pneumonia (males 1,269, females 1,097) and disorders of the stomach—including ulcers (males 1,111, females 1,019).

But the importance of the Social Survey is not so much the numerical results obtained from it as the fact that it was the first stage of a new emergence of interest in the development of morbidity statistics. Previously, information about sickness had been limited to particular diseases, and to the experience of a few specially studied community groups. The Social Survey at once extended the information to all causes of illness and the scope of enquiry to the population at large. The potential value became well recognized but the limitations of the social survey data animated both the exploration of other sources of information and the development of methods providing data more precise in a nosological sense. Not only the authors of this final report, but all who played a part are owed a tribute of praise for what has subsequently developed.

E. Lewis-Faning.

16.—*The Bethlem Royal Hospital and The Maudsley Hospital. Triennial Statistical Report, 1952–54.* Edited by C. P. Blacker. 152 pp. 8½″. 10*s.* 6*d.*

This is the second report in the series; the first, covering the period 1949–51, was reviewed in this *Journal* in 1956 (page 215). The objects of these reports are (*a*) to record the activities of the hospital and note such changes as may have internal interest and (*b*) to draw attention to trends in psychiatry found from analysing hospital records. The report contains a wealth of material on in- and out-patients covering such items as age, sex, marital status, age at first marriage, religion, size of sibship, fertility, social class, weekly income, occupation, employment, family history, source of reference, diagnosis, treatment, duration of treatment disposal and so on.

The present analysis covers 9,357 adult individuals, of whom 4,411 were males and 4,946 females. In addition, there were 1,146 children (751 boys, 395 girls). The number of adults increased by 20 per cent. on the earlier period while the number of children was 5 per cent. lower.

The data are arranged to facilitate easy comparison between the two trienna and, in time, a comprehensive and valuable picture of the hospital patients will be available for administrative and research purposes.

A. Barr.

17.—*Registrar General's Decennial Supplement, England and Wales,* 1951, *Area Mortality.* London, H.M.S.O., 1958. xxviii, 82 pp. 13¼″. 70*s.*

Regional differences in mortality have been a well-known feature of the experience of England and Wales during the past hundred years. The northern section of the country has always had a higher death rate than the southern, particularly from respiratory diseases, and Wales has had an above average mortality from respiratory tuberculosis. It seems that latitude and climate can only be responsible for a very small part of the regional differences in mortality. Among the factors that influence the south/north distribution of mortality are the locations of industries, the density of the population, the differences in social classes and occupations. Certain industries must be carried out in particular areas, coal mining, ship building, etc., and the availability of the raw materials also plays an important part in the siting of industries such as the iron and steel, pottery, etc. Climatic

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conditions favoured the establishment of the cotton and wool industry in Lancashire and the West Riding of Yorkshire. During the present century great efforts have been made to lessen the environmental risk and in addition many of the gross industrial risks have been eliminated but although the death rate has fallen considerably the relative regional differences in mortality are still large.

For the first time the Registrar General has devoted a separate volume to mortality by area, the death rates being based on the average of the four years 1950–53. For this period the largest standardized mortality ratios (S.M.R.) from all causes of death were, for males, 111 in the north-western region, 108 in Wales, and 107 in the northern region. For females the largest ratios were 110 in the north-west region, 109 in the northern and 108 in Wales. The lowest S.M.R.'s for males were 89 in the eastern, 92 in the southern and 95 in the north midland region and for females the lowest ratios were 93 in London and south-east, 93 in the southern and 94 in the eastern region. For the density areas the lowest ratios were those of the rural districts, 90 for males and 97 for females, and the highest male ratio was 116 in the Merseyside conurbation and the highest female ratio was 112 in the south-east Lancashire conurbation. The male mortality from respiratory tuberculosis was largest in Wales, 124, and the midland region, 117, and lowest in the eastern region, 64, and the southern region, 79, and for females the highest ratios were in the northern region, 136, and Wales, 134, and the lowest in the southern, 72, and eastern region, 74. The S.M.R. for cancer of the stomach was largest in Wales, 129 for males and 135 for females, and lowest in the southern region, 84 for males and 81 for females. The highest death rate from cancer of the lung was that of London and south-eastern with a S.M.R. of 123 for both males and females and the lowest ratios were 76 for males in the south-western region and 72 for females in Wales. Females in London and south-east had the highest mortality from cancer of the breast, 107, and the lowest mortality from cancer of the uterus, 89, while the northern region had the lowest rate from cancer of the breast, 86, and the highest from cancer of the uterus, 118. Pneumonia mortality was highest in London and south-east for both sexes and Wales had the lowest mortality. The largest death rates from bronchitis were in the north-western region and in the East and West Ridings. The S.M.R.'s for arteriosclerotic heart disease including coronary disease were largest for both sexes in the northern region, 113 for males and 126 for females, and lowest in the midland region, 88 for both sexes. The southern region had the largest male ratio for motor vehicle accidents, 119, and the lowest female ratio, 91. The male S.M.R. for motor vehicle accidents in the rural districts was 137 compared with 87 in the conurbations.

The stillbirth rate, neonatal mortality and the infant mortality were lowest in London and south-east and the southern region and highest in the north, East and West Ridings and the north-western region. There was a considerable variation in the infant mortality within each type of area, in the county boroughs it ranged from 17 in Eastbourne to 43 in Rochdale and Merthyr Tydfil, in the administrative counties the range was from 20 in Cambridgeshire to 43 in Anglesey, and in the Metropolitan boroughs from 20 in Finsbury, Greenwich and Hammersmith to 30 in Westminster. W. J. MARTIN.

18.—*Registrar General's Decennial Supplement, England and Wales*, 1951, *Occupational Mortality*, Part II, Vols. 1 & 2. London, H.M.S.O., 1958. 13″. 147*s*. for 2 vols.

These volumes deal with the occupational and social class mortality experience during 1949–53 of (*a*) males, (*b*) married women, (*c*) single women, (*d*) infants dying under one year of age, and (*e*) stillborn children.

Many differences are shown between the present mortality and that recorded by the preceding analysis relating to the years 1930–32. The mortality rates have been based on the average of deaths occurring in five years, 1949–53, while in preceding reports the average was based on three years deaths. The mortality by social class has undergone a striking change and for the first time the standardized mortality ratio (S.M.R.) does not rise in a steady gradient from class I to class V. The S.M.R. for males for social classes I to V is 98, 86, 101, 94, 118. A part of this difference is due to the change in social clas-

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sification. When the 1930–32 classification is used the ratios are 100, 90, 101, 104, 118. It appears that the apparent improvement in the mortality rate of social class IV is due to changes in the occupations which constitute this social class but the older classification does not affect the relative positions of the other four social classes. A considerable improvement has occurred in mortality since 1930–32 and for males under age 45 the death rates are less than one-half of the level of 20 years earlier. For ages 20–, 25–, 35–, 45–, 55–64 the greatest improvement in mortality has been in social class II and the smallest improvement has been in social class I, except for ages 25–54 in social class V where the fall was slightly less than in social class I. The married women had very much lower mortality rates than the males and their death rates expressed as percentages of the males were 57, 74, 71, 59, 51, 56, 62 for ages 20–, 25–, 35–, 45–, 55–, 65–, 70–74.

Density of population affected the mortality of the males more than that of the married women. For conurbations, urban areas with a population of 100,000 or more, urban areas with a population of 50,000–100,000, urban areas with a population of less than 50,000 and rural areas the S.M.R.'s were 106, 107, 100, 98 and 84 for males and 102, 105, 98, 101, and 90 for married women. The S.M.R. for respiratory diseases in males was 118 in the conurbations and 65 in rural districts and for married women the values were 112 and 77 respectively. The geographical distribution showed the largest S.M.R. in the north-western region, 115 for males and 114 for married women, and the lowest were in the eastern region, 82 for males and 88 for married women. Greater London was the healthiest of the conurbations with a S.M.R. of 97 for males and 91 for married women; Merseyside was the worst for males with a ratio of 124 and Tyneside was the worst for married women with a ratio of 127.

There was no marked social class differential in the mortality from all sites of cancer but several individual sites showed a definite relationship with social class, some downward and some upward gradients from social class I to V. The S.M.R. for vascular lesions of the central nervous system was highest for males in social class I (124) and lowest in social class IV (88). The male mortality from coronary disease was highly correlated with social class, the mortality in social class I was almost 50 per cent. above the average. No clear evidence of a social class gradient was shown by the married women's mortality from coronary disease. Chronic rheumatic heart disease showed a steep social gradient from class I to V for both males and married women. Influenza, pneumonia, bronchitis, and bronchiectasis each showed a steep gradient from class I to class V for males, married women and single women. The S.M.R. for ulcer of the stomach in males rose from 53 in social class I to 144 in social class V and ulcer of the duodenum rose from 81 to 125. Among females ulcer of the stomach had a marked gradient with social class but ulcer of the duodenum did not show any evidence of a social class correlation. The male mortality from cirrhosis of the liver was over twice as high in social class I as in social classes IV and V. The suicide rate was highest in social class I for males, married women and single women.

As usual the infant mortality showed a gradient with social class, the rates for social classes I to V were 18·7, 21·6, 28·6, 33·8 and 40·8 per 1,000 live births. The splitting of these deaths into the first four weeks of life, the neonatal period, and those related to the remainder of the first year of life, the post-neonatal period, showed that most of the mortality was in the first four weeks of life. The neonatal mortality rate was higher than the post-neonatal mortality rate in each social class but the steepness of the gradient from class I to class V was mainly influenced by the post-neonatal deaths. It seems that for the principal causes of death in the first four weeks of life the mortality is not so influenced by social class as those deaths which occur in the remainder of the first year of life.

W. J. MARTIN.

19.—*Shipbuilding in Britain, Mainly between the two World Wars.* By L. Jones. Cardiff, University of Wales Press, 1957. [vii], 244 pp. 8½″, 30*s*.

This study by Mr. Leslie Jones is greatly to be welcomed for it brings together under one cover a large variety of facts which previously were virtually inaccessible amongst trade and

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other publications. Moreover, Mr. Jones writes fluently and with skill so that very little effort is required to assimilate the facts or follow the arguments he advances. But the composition of the book inevitably suffers from the paucity of information available about the industry in the inter-war period. The industry established the Shipbuilding Conference in 1928 and the Shipbuilding Employers' Federation long before that, but neither has developed that body of published information that is so essential to the research worker. In consequence a great deal of the book has had to be concentrated on the demand for ships and the factors affecting it so that at times the reader wonders whether it is shipping and not shipbuilding that he is reading about.

Mr. Jones deals at some length with the operation of National Shipbuilders' Security in purchasing and dismantling shipyards. It is possible to view it more dispassionately in the 1950s than in the 1930s; perhaps it was not really a very important incident though it may be thought that the money used for the purpose could have gone a long way to reorganizing one or two yards. Likewise the State Aid offered to tramp shipping appeared to have very little effect on the building of new ships. The problems of an industry, however, become immensely aggravated once it begins to go down hill; not only is it unable to resist depression but it is also unable to profit from a boom, a point brought out in the examination of output in the years before the second world war.

Probably the best chapter in the book is that on foreign competition in shipbuilding though the rhetorical questions posed on page 62 remain unanswered: It is simply not possible to decide precisely why the share of the U.K. in the world market for ships fell from about 50 per cent. in much of the 1920s to 40 per cent. or less in the 1930s. It appears that in the 1930s steel prices were lower than in Germany and France though higher than dumping levels; no doubt subsidies abroad, higher wages at home and the loss of technical initiative all contributed, but it is to be suspected that the root of the trouble lay in low productivity and the difficulties facing management in attempting to remedy it in this country, and in this respect Mr. Jones's comment that diesel machinery cost very much (how much?) less on the Continent than in this country is revealing. If this is the case, however, why is it that the Doxford Company appears to have been given pride of place in the development of the diesel engine and not Burmeister & Wain (page 40)?

Some reference to productivity and management's responsibility for it would have been welcome in the final chapter which presumably attempts to bring up to date a study concerned primarily with the inter-war period. This chapter is helpful in some ways but it is marred by Mr. Jones's inability, when assessing future demand, to make up his mind as to how long a ship lasts. Is it 25 years, implying a replacement rate of 4 per cent., or fifty years corresponding to the 2 per cent. wastage rate experienced by the world in the inter-war period? And is it really justifiable to assume that the world dry cargo fleet henceforward will be static? The answers given to these questions make a great deal of difference to the future outlook for shipbuilding.

On other points of detail some criticisms may be felt. It would have been interesting on cyclical fluctuations to have had the views of Mr. Jones on the work of Einarsen, Koopmans, Tinbergen, or D. H. Robertson; and it would have been more comforting if the three-shift system had not been quite incorrectly described as almost universal abroad; finally, would Mr. Jones agree that output per man in Sweden is 50 per cent. greater than on the average in this country?

J. R. PARKINSON.

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STATISTICAL NOTES

(1) British Official Statistics

The index of retail prices compiled by the Ministry of Labour and National Service which was 108 in February and March rose to 110 in April and fell to 109 in May. In March there were increases in the average prices of sugar, bacon and potatoes. There were some further increases for bacon and potatoes in the following month and also some rises in rents and rates. The fall in June was largely due to reductions in the prices of coal, milk and eggs. The detailed figures of the weights used in calculating the index and the indices for different commodity groups were as follows :

	Food	*Alcoholic Drinks*	*Tobacco*	*Housing*	*Fuel and Light*	*Durable Household Goods*	*Clothing and Footwear*	*Transport and Vehicles*	*Miscellaneous*	*Services*	*All Items*
Weights:	350	71	80	87	55	66	106	68	59	58	1,000
Feb. 18th, 1958	103·9	105·8	107·8	118·1	115·5	101·4	103·3	112·8	112·4	113·2	107·6
Mar. 18th, 1958	105·7	105·8	107·8	118·4	115·6	101·5	103·4	112·8	112·6	114·2	108·4
Apr. 15th, 1958	108·7	105·6	107·8	120·6	115·7	101·3	103·3	112·9	113·1	114·3	109·6
May 13th, 1958	108·5	105·6	107·8	121·3	109·9	100·2	103·1	112·9	112·7	114·4	109·2

The Ministry of Labour index of weekly wage rates, calculated on the basis of January 31st, 1956 = 100, showed that in February the level was 112·6. It rose to 112·7 in March and April and to 112·8 in May. The principal classes of workpeople whose rates of wages rose in the period included wool textile operatives, bleachers, dyers and finishers, pottery workers, dressmakers, tobacco workers and employees in the cocoa, chocolate and confectionery trades.

Statistics derived from the six monthly enquiry into earnings show that in October 1957 31 per cent. of the workers in all the industries and services covered by the enquiry were paid under systems of payment by results. This compares with 25 per cent. in October 1938 and 28 per cent. in October 1947. In manufacturing industries alone the percentages were 39, 33 and 37.

The total working population and the numbers in civil employment in the four months ended April 1958 were as follows :

(*Thousands*)

Dates	*Total Working Population*			*Numbers in Civil Employment*		
	Males	*Females*	*Total*	*Males*	*Females*	*Total*
Jan., 1958 . .	16,210	7,914	24,124	15,303	7,790	23,093
Feb., 1958 . .	16,188	7,889	24,077	15,280	7,765	23,045
Mar., 1958 . .	16,176	7,872	24,048	15,274	7,748	23,022
Apr., 1958 . .	16,178	7,890	24,068	15,283	7,763	23,046

The number of persons on the unemployment registers of the Employment Exchanges rose by 8,500 in March, 10,600 in April and 3,900 in May. The total for May represented 2·1 per cent. of the total number of employees. The percentages in the different Regions ranged from 1·3 in London and South Eastern and 1·4 in the Midlands to 3·6 in Scotland and 3·9 in Wales.

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The following is a sex analysis of the figures :

Number of Unemployed Persons on the Registers of Employment Exchanges

	Men and Boys	*Women and Girls*	*Total*
Feb. 17th, 1958 . . .	301,532	123,015	424,547
March 17th, 1958 . . .	306,981	126,090	433,071
April 14th, 1958 . . .	313,972	129,727	443,699
May 12th, 1958 . . .	309,416	138,140	447,556

Of the total of 447,556 in May, 88,358 had been unemployed for less than two weeks, 104,643 for two to eight weeks, and 196,043 for over eight weeks, while 58,512 were temporarily stopped. In the four weeks ended May 7th, 1958, 157,964 vacancies were filled by the Employment Exchanges. The number of vacancies at this date was 207,589.

The numbers of insured persons absent from work owing to sickness, including self-employed as well as employed, were 1,053,500 in February, 992,200 in March, 929,500 in April and 851,300 in May. The numbers of employed persons absent owing to industrial injuries, were 57,000, 59,400, 55,700 and 58,300.

The *Ministry of Labour Gazette* for June 1958, contains an age analysis of the employee population in Great Britain at May 1957. Of the total of 14,200,000 males 195,000 were 70 and over, and 597,000 were 65 and over. Amongst 7,650,000 females 48,000 were 70 and over and 162,000 were 65 and over. Figures relating to married women showed a total of 3,770,000, or nearly one-half of the total number of female employees. This was an increase of less than 50,000 over the figures for 1956, compared with very large increases in the preceding two years. The review also includes regional and industrial analyses of the figures and some estimates of inter-regional migration.

(2) Other Statistics

The Economic Commission for Europe's annual *Economic Survey of Europe for* 1957 (390 pp., 28*s*.) in addition to the usual survey of trade, production etc. contains two special features. The first is an attempt to provide a comprehensive picture of post-war trends in the size and structure of the labour force of eastern Europe and U.S.S.R., made possible by recent improvements in data from these countries. It concludes that the former ample supply of, and demand for, labour in most of these countries has given way to a new pattern with labour surpluses appearing in some countries and shortages in others. The second is an analysis of the interrelations of expenditure and output in O.E.E.C. countries by determining the amount of each category of resources (net product) going into each end-use sector (home demand plus exports). Of the total resources in 1953 at current prices (estimated in thousand million dollars at 209·8) 23·1 came from agriculture, 75·2 from industry, 67·4 from transport and services and 20·4 from imports; while of this total, private consumption accounted for 124·9, public consumption 29·3, gross fixed investment 32·5 and exports 21·1.

In a chapter on western European expansion since 1953, it is pointed out that the rate of growth in 1957 was for the second year below that of 1955 and concludes that if present trends and policies continue there is little prospect of the recent expansion continuing at anything like the same pace in 1958.

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The *United Nations Statistical Yearbook* 1957 (674 pp., cloth 57*s.*, paper 46*s.*) in addition to its usual features contains a new chapter on International Economic Aid in 1954–56. Three tables show in U.S. dollars, total grants and loans furnished to under-developed countries by governments and inter-governmental agencies and the amounts *per capita* and per national product. The world index of industrial production has been recalculated. New tables are also given on discount rates of Central Banks and on ocean freight rates, and much more information is available this time on the eastern European countries and U.S.S.R. The world-wide scope of the Yearbook is shown by the fact that the "country index" lists over 250 countries, territories and other areas.

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STATISTICAL AND ECONOMIC ARTICLES IN RECENT PERIODICALS

UNITED KINGDOM—

Annals of Human Genetics—

Vol. 22 (1958), Part 3—Inheritance of the P.T.C. taste character in man: an analysis of 126 Rárhi Bráhmin families of West Bengal; *S. R. Das.* Muscular dystrophy in Northern Ireland. IV. Some additional data; *A. C. Stevenson.* The sex ratio of mutation rates of sex-linked recessive genes in man with particular reference to Duchenne type muscular dystrophy; *E. A. Cheeseman, S. J. Kilpatrick, A. C. Stevenson* and *C. A. B. Smith.* Estimates of the sex ratio of mutation rates in sex-linked conditions by the method of maximum likelihood; *C. A. B. Smith* and *S. J. Kilpatrick.* A test for birth order effect; *D. E. Barton* and *F. N. David.*

Biometrika—

Vol. 45 (1958), Parts 1 and 2—Numerical studies in the sequential estimation of a binomial parameter; *P. Armitage.* A stochastic model for studying the properties of certain biological systems by numerical methods; *P. H. Leslie.* On the derivation and applicability of Neyman's type A distribution; *J. G. Skellam.* Negative binomial distributions with a common k; *C. I. Bliss* and *A. R. G. Owen.* Simplified methods of fitting the truncated negative binomial distribution; *W. Brass.* The interpretation of the effects of non-additivity in the Latin square; *D. R. Cox.* Quantal responses to mixtures of poisons under conditions of simple similar action: the analysis of uncontrolled data; *J. R. Ashford.* Some properties of runs in quality control procedures; *P. G. Moore.* Simultaneous regression equations in experimentation; *E. J. Williams.* One-way variances in a two-way classification; *T. S. Russell* and *R. A. Bradley.* Studies in the history of probability and statistics. VII. The principle of the arithmetic mean; *R. L. Plackett.* Multivariate linear structural relations; *R. L. Brown* and *F. Fereday.* Multivariate ratio estimation for finite populations; *I. Olkin.* Non-randomness in a sequence of two alternatives. I. Wilcoxon's and allied test statistics; *D. E. Barton, F. N. David* and *C. L. Mallows.* Simplified runs tests and likelihood ratio tests for Markoff chains; *L. A. Goodman.* Moment generating functions of quadratic forms in serially correlated normal variables; *R. B. Leipnik.* Moments of sample moments of censored samples from a normal population; *J. G. Saw.* The relation between the dictionary distribution and the occurrence distribution of word length and its importance for the study of quantitative linguistics; *G. Herdan.* Second paper on statistics associated with the random disorientation of cubes; *J. K. Mackenzie.* Conditioned Markov processes; *W. A. O'N. Waugh.* Ranking means of two normal populations with unknown variances; *R. Maurice.* Non-randomness in a sequence of two alternatives. II. Runs test; *D. E. Barton* and *F. N. David.* Note on multiple comparisons for adjusted means in the analysis of covariance; *M. Halperin* and *S. W. Greenhouse.* An empirical investigation into the distribution of the F-ratio in samples from two non-normal populations; *H. R. B. Hack.* Theoretical considerations regarding H. R. B. Hack's system of randomization for cross-classifications; *N. L. Johnson.* On the equivalence of two tests of equality of rate of occurrence in two series of events occurring randomly in time; *D. E. Barton.* The mathematical relation between Greenberg's index of linguistic diversity and Yule's characteristic; *G. Herdan.* Note on a discontinuous probability density; *J. E. Kerrich.* A remark on Spearman's rank correlation coefficient; *H. Bergström.* Interval estimation for the parameter of a binomial distribution; *C. W. Cluniès-Ross.* Further critical values for the sum of two variances; *A. Huitson.* Studies in the history of probability and statistics. VIII. De Morgan and the statistical study of literary style; *R. D. Lord.*

British Journal of Preventive and Social Medicine—

Vol. 12 (1958), No. 2—The extent of hospital sickness; *A. Barr.* Morbidity in the insured population of Northern Ireland; *A. T. Park* and *C. W. Kidd.* Mortality from coronary artery disease and "myocardial degeneration" in the Republic of Ireland; *R. M. Acheson*

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and *E. H. Thornton.* Air pollution and other local factors in respiratory disease; *A. S. Fairbairn* and *D. D. Reid.* Atmospheric pollution and mortality in the county of London; *A. T. Gore* and *C. W. Shaddick.*

Bulletin of the Oxford University Institute of Statistics—

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Vol. 77 (1958), No. 4—Effects of the European common market on employment and social conditions in agriculture; *H. Niehaus.*

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LIST OF ADDITIONS TO THE LIBRARY

Since the issue of Part 2, 1958, the Society has received the publications enumerated below.

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United Kingdom. *Admiralty.* Subtabulation: a companion booklet to *Interpolation and allied tables*; prepared by H.M. Nautical Almanac Office, published by order of the Lords Commissioners of the Admiralty. London, H.M.S.O., 1958. 54p. 9¾″. 7*s.* 6*d.*

Air Ministry. Meteorological Office. Tables of temperature, relative humidity and precipitation for the world. Part I. North America, Greenland and the North Pacific Ocean. Part IV. Africa, the Atlantic Ocean south of 35° N. and the Indian Ocean. Part V. Asia. Part VI. Australasia and the South Pacific, including the corresponding sectors of Antarctica. London, H.M.S.O., 1958. 4 pts. fold.maps. 9¾″. 45*s.* 6*d.* (M.O. 617a, d, e, f)

Central Office of Information. Education in Britain. [2nd ed.] London, H.M.S.O., 1958. [iv], 59p. 9¼″. 3*s.* 6*d.* (Ref. Pamphl. 7)

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Commonwealth Economic Committee. A review of commonwealth raw materials. Vol. 1. London, H.M.S.O., 1958. viii, 288p. 9¾″. 15*s.* (37th Report)

Council on Prices, Productivity and Incomes. First report. London, H.M.S.O., 1958. iv, 75p. 9¾″. 2*s.*

Department of Scientific and Industrial Research. Research for industry: a summary of work done by the industrial research associations in the government scheme, 1956–57. London, H.M.S.O., 1958. iv, 159p. 9¾″. 7*s.* 6*d.* Previously included in the Department's *Report for the year . . .*

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The Development of the Universities in Great Britain

By Sir Keith Murray

[Read before the Royal Statistical Society, May 14th, 1958,

Mr. R. F. George, Vice-President, in the Chair]

I. *Introduction*

It was with very considerable hesitation that I accepted an invitation from your Society to write a paper on the Universities for the *Journal* of your Society. The days when I had a working knowledge of statistics have long passed and I can no longer claim to be an academic. I did, however, recall from those early days when I consulted the *Journal* how broad was the scope of its contents, including papers and discussions which were not severely statistical but simply matters of public moment. This recollection encouraged me to think that a survey of the recent history of the Universities in Great Britain and of a few of the problems of the next decade might be of interest to your members. I should add that the views expressed in this paper are my personal ones for which the members and other officials of the University Grants Committee cannot be held responsible.

It is necessary first to clarify what is meant by Universities. In the present context the term means those institutions which receive grants from the Vote for Universities and Colleges, etc. Great Britain (No. 21 of Class IV of the Civil Estimates). The provision which Parliament makes for these grants is made as a single lump sum, known technically as a "grant-in-aid". This sum is paid into a deposit account from which grants are made to the various institutions. The Treasury is the Department of Government responsible for Government policy in relation to university finance. The Chancellor of the Exchequer decides how much in total to ask Parliament to provide for the Universities. He decides this in the light of advice tended to him by the University Grants Committee, which is appointed by him, but the decision is his own. The Treasury operate the deposit account into which the grant-in-aid is paid and account to Parliament for the Vote, but the payments of grants which they make from the deposit account are made on the recommendation of the University Grants Committee and Parliament are informed that this is the position. The Treasury thus publicly delegate to the University Grants Committee the decision what institutions are to receive grants from the provision made by Parliament for University grants and how the total provision is to be shared out between them. The effect of these arrangements is that while the University Grants Committee is an advisory body in reality,

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as well as in form, in relation to the total provision for university grants, it is in effect an executive body for the allocation of that provision among the Universities.

There are in all twenty-one Universities and three Colleges which are grant-aided through the University Grants Committee. They fall, almost chronologically through the dates of their Charters, into seven categories:

(1) Oxford and Cambridge Universities.
(2) The Scottish Universities.
(3) London University, Durham University and the University of Wales, the federal Universities consisting respectively of thirty-nine, two and five constituent parts.
(4) The seven large civic Universities, Manchester, Birmingham, Liverpool, Leeds, Sheffield, Bristol and Nottingham.
(5) The five smaller civic Universities, Reading, Southampton, Hull, Exeter and Leicester.
(6) The Manchester College of Science and Technology and the Royal College of Science and Technology, Glasgow, whose students take respectively the degrees of the Universities of Manchester and Glasgow.
(7) The University College of North Staffordshire.

The principal features of each of these groups and institutions can be found in detail in the Reports of the University Grants Committee (1953). It is sufficient for the purpose of this discussion to emphasize that they differ in numerous respects, in origin and date of foundation, in size, in government and administration, in coverage of subjects, in their curricula and approach to their subjects, in their teaching methods and in the development of their research, and in their social and economic environment. It cannot be said, even within the categories given above, that there is any evidence of a clearly-defined pattern or of the dead-hand of uniformity.

The second point which should be noted is the wide distribution in their size. Omitting the Universities of London, Oxford and Cambridge, and counting separately the Divisions of Durham University and the constituent Colleges of the University of Wales, none has over 6,000 students, while sixteen have under 2,000 (see Table 4).

II. *The Post-war Years*

The developments of the post-war years have had a profound effect on the Universities. Social changes, the expansion of secondary education and the extension of the system of awards have almost doubled the numbers of those receiving a University education. The growth of science and technology has meant a major re-orientation of university resources, both in teaching and in research. The needs of the modern State for an increasing number of graduates, particularly in certain fields, has added materially to the responsibilities of the Universities. Finally, the State has assumed an increasingly important role in the financing of University education.

Changes in the Student Population

In 1938–39, there were about 50,000 full-time students in the Universities and Colleges and in the immediate post-war years the number rose to over 85,000; but it then declined to just under 81,000 in 1953–54 as the wave of ex-Servicemen passed. In the succeeding

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five years the number has, however, risen again, to 90,000 in 1956/57 and 94,600 in October 1957, in spite of the fact that the age group from which these students were drawn was small as a result of the low birth-rate in the 'thirties.

These changes are in the main the result of three factors. Firstly, the proportion of children staying on at school to 17 or 18 years of age and qualifying for admission to the University has been growing steadily. In pre-war years, fewer than 6 per cent. of the 17–18 age group in England and Wales remained at school to 17 years or over; in 1952 this proportion had risen to 8·2 per cent. and in 1956 to 10·5 per cent. The proportion of any age group which reaches the Universities is now about 1 in 30 compared with about 1 in 60 before the war. This development, known as "the trend", is adding 4,000 to 5,000 students a year to University numbers. The second factor has of course been the very great increase in student awards, first under the Further Education and Training scheme and subsequently from the Ministry of Education and Local Authorities. The proportion of assisted students, i.e. students holding scholarships, exhibitions or other awards, whether from public or private funds, which provide wholly or in part for the payment of their fees or other expenses, rose from 41·1 per cent. in 1938–39 to 75·7 per cent. in 1956–57. It is perhaps worthy of note that not only is the proportion of unassisted students declining but the absolute number has dropped in recent years, having fallen from 24,129 in 1952–53 to 21,824 in 1956–57 at a time when total student numbers were rising rapidly.

The third factor is the slow but definite increase in the length of time taken by students to achieve the qualifications which they want. This is generally due to their taking an additional degree or diploma after their first degree, rather than to any marked tendency for the duration of undergraduate courses to lengthen.

The second point of note about the changing student population lies in the subjects which students are studying. What a student studies in this country is, fortunately, still his own choice or, at second best, that of his parents or school-teachers. Within certain limits, therefore, the Universities have to cater for what the student chooses. The numbers studying some form of Science have increased from about 27,000 in 1938–39 to about 54,000 in 1957–58.

TABLE 1

Numbers of Students in Different Faculties in the Universities, 1938–39 *and* 1957–58*

Faculty	1938–39	1957–58	*Per Cent. of Total* 1938–39	1957–58
Arts	22,374	40,663	44·7	43·0
Pure Science	7,661	21,333	15·3	22·6
Technology	5,288	13,730	10·6	14·5
Medicine and Dentistry	13,636	15,729	27.3	16·6
Agriculture and Forestry	1,043	1,981	2·1	2·1
Veterinary Science	—	1,164	—	1·2
Total	50,002	94,600	100·0	100·0

* October, 1957.

At the present time 43 per cent. of those entering the Universities are reading Arts, including Social Studies, and the remaining 57 per cent. are studying Pure or Applied Sciences including Medicine, Dentistry, Agriculture, Forestry and Veterinary Science. It

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is true that these percentages do not show any appreciable change from the pre-war years when the Arts percentage of the total student numbers studying Arts was 44·7 per cent.; but the comparison of the pre-war and post-war total percentages is somewhat misleading in that there have been certain changes in the nature of what is included in Arts and in the length of courses. A clearer picture is probably given by the total number of first degrees awarded in Arts and in the other subjects. They are as follows:

TABLE 2

Change in the Proportion of First Degrees in Arts to the Total Number of First Degrees 1938–39 *to* 1955–56

	Total Number of First Degrees Awarded	*Total Number of First Degrees Awarded in Arts Faculties*	*Per Cent. of Total*
1938–39	9,311	4,895	53
1955–56	16,785	7,907	47·1

The recent relative swing in numbers from Arts to Science has been more marked than many people imagine and it would appear likely to continue. In 1956 the number of passes in all subjects at Advanced Level in the General Certificate of Education was 38 per cent. higher than in 1951; the increase in Mathematics and Physics, however, was 51 per cent. It has also been suggested that this swing is not only in quantity but also in quality and some evidence of this may be found in the comparison in the number of first classes in Arts and Science given in seven Universities between 1936–38 and 1952–54. The first fact to emerge was that while there had been an increase of over 70 per cent. in the total number of students, the total number of firsts had increased by only 9 per cent. But the difference between Arts and Science was even more striking; there was an absolute decrease in the number of the former, the percentage falling by 17 per cent. while the number of firsts in Science and Technology had risen by 35 per cent.* A certain amount of scepticism may be raised about this comparison, but the figures are suggestive.

The expanding student population is, however, not merely a problem of numbers and subjects studied. The student of today differs in many ways from the student of twenty or forty years ago, and evidence of the change in the social background of students has been given in various surveys carried out in recent years. It is the general view of the Universities that a much higher proportion of their students than formerly are young men and women with few cultural interests and attainments beyond those they have acquired at school, and in recent years an increasing proportion of University students have come from families in which there was no tradition or experience of University education. Students who are the first of their families to reach a University may arrive there with little conception of what a University life has to offer outside its formal curriculum and they may never learn it unless there is a sharp break with home life which compels them to look to the University and their fellow-students for new social contacts and new recreations to replace those which they have left behind.

* Number of first classes:

	1936–38	1952–54
Arts	1,529	1,269
Science (incl. Technology) . . .	1,605	2,167

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There is an increasing tendency on the part of students to move from home when studying at the Universities—a further factor calling for readjustment on the part of the student. The percentage of students in England and Wales who live at home has dropped from 51 per cent. to 28 per cent. in the last seven years. Excluding Oxford and Cambridge, about 22 per cent. of the students live in University residences. The rest are in lodgings. The change in the social and geographical background of the students has forced upon the Universities a number of problems in teaching and in the organization of student life which were not hitherto present or recognized.

In spite of these rapid changes in numbers and background, there is no evidence that academic standards in the Universities have declined, even with the remarkable increase in student numbers that has occurred since the war. It is probably true that the proportion of outstandingly good and outstandingly weak students is lower but that of good second-class is higher.

An elaborate enquiry was carried out by the University of Liverpool into the progress of students who entered that University in the years 1947–49 (Mountford, 1956). First-year failures amounted to only 10 per cent. compared with 18 per cent. before the war. Some 86 per cent. of the students had gained some qualification, less than 1 per cent. had not completed a course and 13 per cent. had abandoned their courses for a variety of reasons before taking their finals. The "wastage" rate is probably more serious than the "failure" rate.

An enquiry by the University Grants Committee into the progress by the end of their first year of students entering Universities in 1950 disclosed a casualty rate of between 8 per cent. and 9 per cent. (University Grants Committee, 1953, p. 26). A similar but somewhat fuller enquiry was undertaken three years later into the progress of students who were admitted for the first time in the academic year 1953–54 to courses for first degrees and diplomas (University Grants Committee, 1957*a*, p. 8). Of the students covered by the return, 87·4 per cent. re-entered in October 1954 and proceeded to the second year without being required to repeat any first-year work; a further 8·4 per cent. were re-entered in October 1954 either to repeat the first year course in whole or in part or to take another course. Only 4·2 per cent. were refused re-admission on account of academic failure or for disciplinary reasons.

A later enquiry by the University Grants Committee into the progress of the 1952–53 entry into Arts, Science and Technology showed that 80·6 per cent. passed their degree examinations within four years and a further 2·7 per cent. were considered likely to graduate; 5·4 per cent. left for non-academic reasons and only 11·3 per cent. left for reasons of academic failure. Compared with failure and wastage rates in other countries, the over-all situation in this country is by no means alarming.

If an increase in the number of University graduates is a matter of great import to the State, it must be considered whether there is an existing wealth of talent that is not given an opportunity of reaching the University, either from personal choice or by failure to find a place in a University.

There seems to be a reservoir of potential students, though a relatively small one, in those who leave school before 18. A report by the Ministry of Education in 1954 estimated that about 5,000 more boys and 5,000 more girls could very well have completed advanced courses leading to the University if they had stayed longer at school (Central Advisory Council for Education, 1954).

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The problem of selection by the Universities has not in the past caused much concern, though it may well do so in the future as competition to get into the Universities increases. The Committee of Vice-Chancellors caused a study to be made into the applications for admissions to the Universities of all those applying for admission in 1955–56 to a full-time course of study leading to a first degree or first diploma (Committee of Vice-Chancellors, 1957). On the whole the enquiry was comforting in its statistics on the number of unfilled places, amounting to only some 2,500. There were many causes of unfilled places; well over half of them were in Arts subjects whereas less than 37 per cent. of those not admitted came within the Arts groups. Again some of the unfilled places were due to the failure of applicants, for whom they had been reserved, to notify the University or College concerned that they were not proposing to fill them until it was too late to offer the places to others.

Academic Staff

There is much that could be said on the question of the academic staff during the past decade but there is space to deal with only one or two of the more important points. The first of these has been the very satisfactory increase in the numbers of academic staff relative to the numbers of whole-time students in the post-war years. The staff/student ratio, excluding Oxford and Cambridge, has risen from 1 : 10·2 in 1938–39 to 1 : 8·0 in 1951–52 and to 1 : 7·2 in 1956–57, having reached its highest point of 1 : 7·0 in 1954–55. This improvement has three great merits. It has enabled the Universities to take more post-graduate students at a time when post-graduate study is becoming increasingly necessary. It has made it possible for University teachers to devote more time to scholarship and research; and, finally, in many Universities it has resulted in a marked improvement in teaching methods; undergraduates can be taught in smaller groups, seminars or discussion groups, or even given tutorials, individually or in twos and threes. It is to be regretted that this development has been so uneven throughout the Universities and even throughout the departments of the same University. There are, of course, great practical difficulties in the way of achieving the low staff ratios in all departments—large or small, honours or pass. But even where a similarly low staff/student ratio obtains in two departments in one University, undergraduate teaching may be organized completely differently; seminars and tutorials in the one; large classes and lectures in the other. One is driven to the conclusion that the limiting factor is not always a shortage of staff but the inclination of the head of the department.

The only other point to which reference should be made is the problem of recruitment. In general, the situation in the past ten years has been good, except in a limited number of fields where outside competition from industry, the Scientific Civil Service, or the medical profession has been particularly attractive, e.g. engineering, physics, and some of the pre-clinical sciences. The general academic salary structure was raised in 1949, 1954 and 1957 to meet increased costs of living or recruitment difficulties. The Universities, in general, dislike salary differentials between university subjects for reasons which are well known and admirable. But these have been accepted to a limited extent. Differential salaries for clinical teachers have been recognized for a long time; they were also introduced for pre-clinical and non-medical teachers in 1949, though they were subsequently eliminated in the last salary revision in 1957. Most Universities have also accepted the

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fact that differential salaries may be necessary within the professorial class in certain fields if they are successfully to compete for the calibre of teacher or researcher that they require.

Accommodation

The problem of accommodation has been a particularly critical one in the post-war years. Capital grants by the University Grants Committee were practically non-existent in the pre-war years and as early as 1936 the Committee were drawing attention to "the building crisis of the Universities" and the inadequacy of their accommodation to house their 50,000 students. In 1947, by which time the Universities were attempting to cope with over 80,000 students, their needs for new buildings were estimated at £80 million for the next ten years. Rising costs over the period would have necessitated the expenditure of £107 million to provide the same quantum of building. Actual expenditure on buildings was, for a variety of reasons, only £37 million—under £4 million a year, rather less than one-third of the Universities' requirements. One example will serve to show how this deficiency shows itself in practice. In one institution the science and engineering faculties had a pre-war working space figure of 580 sq. ft. per student. By 1955, though the total floor space had been increased by 7 per cent., the expansion in student numbers had reduced the working space figure to 343 sq. ft. per student.

The University building programme was, however, raised to £10·4 million for 1957 and to £12 million in each of the years 1958 and 1959. Of the expenditure on major buildings, 56 per cent. is devoted to accommodation for science and technology, and 19 per cent. for student residence, refectories, unions and common rooms.

Finance

The last part of this section must deal briefly with the major changes in University expenditure and income during the post-war period.

The growth of University recurrent expenditure since 1938–39 has been phenomenal. In that year, the total recurrent expenditure of the Universities amounted to £6¾ million; in 1944–45, it was about £7¾ million; and by 1955–56 it had risen to £38½ million.

As striking as the total increase in recurrent expenditure has been the change in the sources of University income, as indicated in the accompanying table. The most out-

TABLE 3

Summary and Sources of University Income
1938–39 *and* 1955–56

	1938–39		1955–56	
	£000	*Per Cent. of Total*	£000	*Per Cent. of Total*
U.G.C. recurrent grant	2,078	30·9	26,235	68·4
Tuition fees, etc.	1,999	29·8	4,270	11·2
Payments for research—				
Government Departments	— *	— *	1,245	3·2
Other bodies	— *	— *	1,062	2·8
Endowments	1,036	15·4	1,532	4·0
Grants from Local Authorities	606	9·0	1,086	2·9
Other Parliamentary grants	322	4·9	751	2·0
Other sources	671	10·0	2,136	5·5
Total	6,712	100·0	38,317	100·0

* Included in other sources.

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standing change is, of course, the great increase in the dependence of the Universities on the Treasury grant which has risen from £2,078,000 to £26,235,000, or from 30·9 per cent. of the total income to 68·4 per cent. (see also Table 5).

State assistance for capital expenditure has also increased since before the war, when it was less than £100,000 a year, to £7,082,000 in 1955–56. This sum covers not only the cost of buildings but also fees, sites and equipment for new departments.

The growth in magnitude of these sums over the years is to some extent a reflection of a change in the values of money. But even in terms of real value, the increase in the resources which the State has put at the disposal of the Universities is very considerable. On the assumption that the increase in retail prices between 1947–48 and 1957–58 has been about 62 per cent., the real value of the Universities' recurrent and non-recurrent grants, which have risen from £11·09 million to an estimated £46·64 million (Table 5), has been increased by about 160 per cent. in the ten-year period, or 138 per cent. if only recurrent expenditure is considered. These figures may be compared with a rise in the national income over the same period of about 35 per cent.

III. *The Increasing Responsibilities of the Universities*

The most urgent demand made upon the Universities of today is for the provision of sufficient graduates. Modern society calls for more and more highly educated people all along the line and, in particular, for more and more graduates of an increasing variety of kinds. Industry and commerce, government and public administration and all the services of the welfare state—education, health and the other social services—call for more graduates. The signs are clear that this demand will continue to increase though not necessarily at the same rate in all fields.

But education is only one of the central aims of a University. Research is another. Pure research is generally best done in Universities and, in fact, the greater part has in recent generations found its home in the Universities—by men and women moved simply by intellectual curiosity, wishing to discover new knowledge for its own sake. This is as true of the Sciences as of the Arts.

There is not time to discuss in this paper the balance between teaching and research in the University. The assumption that research and teaching must be in conflict is surely false. While research and teaching must inevitably compete for the time of every University teacher, they should be regarded not as conflicting but as complementary activities. Good teaching cannot often live without research; without research, teaching of the able young tends to become stale and unprofitable.

The Universities may reasonably claim that they have met and are meeting the needs of the State in both their functions of teaching and research. Until 1946, the most important duty of the University Grants Committee was:

> "To enquire into the financial needs of University Education in the United Kingdom and to advise the Government as to the application of any grants that may be made by Parliament to meet them".

Recognition of the change in the relationship between the State and the Universities in the post-war era was indicated by the following addition which was made in 1946 to the Committee's terms of reference:

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> "and to assist in consultation with the Universities and other bodies concerned the preparation and execution of such plans for the development of the Universities as may from time to time be required in order to ensure that they are fully adequate to national needs".

The Universities have recognized their obligations and in July 1946 they assured the Chancellor of the Exchequer that

> "the Universities entirely accept the view that the Government has not only the right, but the duty to satisfy itself that every field of study which in the national interest ought to be cultivated in Great Britain is in fact being cultivated in the University system and that the resources which are placed at the disposal of the Universities are being used with full regard both to efficiency and to economy".

The history of University development in the last ten years shows that this assurance has been honoured.

The needs of the State in particular fields which the Universities have accepted since the war have resulted generally from the recommendations of departmental Committees. The following nine Committee reports indicate the range of these responsibilities:—

(1) The Goodenough Committee (1944) advocated the expansion of medical education in the Universities and in particular the further development of full-time teaching and research. Remarkable progress has been achieved.

(2) The Loveday Committee (1944) brought veterinary education into the Universities who are now turning out over 200 graduates a year and building up strong schools of research.

(3) The McNair Committee on Teachers (1944) placed further responsibilities in the Universities in regard to the education of teachers both within and without the Universities.

(4) The Barlow Committee (1946) recommended the doubling of the University output of scientists and technologists in the next ten years together with a substantial increase in Arts. The target was in fact achieved in five years, when the number of full-time students in Science and Technology was increased from 13,000 before the war to 27,000 in 1952–53.

(5) The Loveday Committee (1946) on Higher Education in Agriculture added to the commitments of the Universities in this field.

(6) The Teviot Committee (1946) brought dental education into the Universities and recently the McNair Committee (1957) has urged that the dental entry into the clinical years, which is now about 660 a year, should be raised to 1,000.

(7) The Devonshire Committee (1946) delegated increased responsibilities to certain Universities for the training of entrants into the Colonial Service.

(8) The Clapham Committee (1946) recommended an expansion of social and economic research within the Universities.

(9) The Scarborough Committee (1946) urged increased teaching and research in the Universities in Oriental, African, Slavonic and Eastern European studies.

Finally, there has been, since 1952, a further demand for more graduates in Science and Technology. In 1952 the Chancellor of the Exchequer invited the University Grants

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Committee to advise how best rapidly to build up at least one institution of University rank devoted predominantly to the teaching and study of the various forms of technology. From this enquiry stemmed the Government decision to double the size of the Imperial College of Science and Technology in ten years, at an estimated capital cost of about £15 million. Again, in July 1953, the Committee was invited, in consultation with the Universities and Colleges concerned, to work out plans for the development of higher technological education outside London and as a result major developments are taking place in Glasgow (at both the University and the Royal College of Science and Technology), Manchester (at both the University and the newly incorporated College of Science and Technology), Leeds and Birmingham, and developments on a fairly large scale at Bristol, Cambridge, Sheffield and elsewhere. The results can be noted in the increase in the number of full-time students in both Pure and Applied Sciences. As compared with 28,000 students in 1954–55, the numbers in 1957–58 are now over 35,000, an increase of 25 per cent. in three years. Much of the increase has been undertaken in anticipation of new accommodation to be erected.

Finally, in 1956, the report of the Scientific Manpower Committee, which was accepted by the Government, gave the Universities and Technical Colleges the target of doubling their 1955 output of professional scientists and technologists by the late 'sixties, i.e. about 20,000 compared with the existing level of about 10,000 (Ministry of Labour, 1956).

While the Universities have assumed these additional undergraduate responsibilities, post-graduate work has also expanded. During the time that the total number of students increased 70 per cent., the number of whole-time advanced students (excluding students taking the Diploma in Education) trebled between 1938–39 and 1955–56; the number of part-time advanced students increased 2¼ times.*

One feature of the post-war years, particularly in the last quinquennium, has been the development of post-graduate instructional courses in science and technology. These courses are intended both for those who have had experience in industry and for those who wish to "top-up" their undergraduate course with more specialist work. The Department of Scientific and Industrial Research last year recognized about 140 of such courses for holders of their post-graduate training awards and the number is increasing rapidly. For example, the Imperial College of Science and Technology will next year offer 41 separate courses including such specialist subjects as automatic control systems, nuclear power and gas turbine technology.

Expenditure of manpower and money on research has also increased in the post-war years, again particularly in the pure and applied sciences. The most recent estimate of expenditure on scientific research carried out in the Universities in the fields of pure science, technology, medicine, agriculture and veterinary science is about £20 million, an increase of about £5·5 million in the last three years. In terms of scientific manpower, the number of teachers, fellows, students and assistants engaged on research has been estimated to be equivalent to about 9,000 full-time research workers. In the opinion of the Advisory Council on Scientific Policy (1957, p. 9), the diversion of resources to research in the Universities is about right in relation to competing needs outside the Universities.

* At the present time the ratios of higher degrees to first degrees are as follows:—

Arts	1 : 8
Technology	1 : 5
Pure Science . . .	1 : 4

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IV. *The Next Decade*

The future development of the Universities is dominated by the potential increase in the number of persons who are likely to qualify for admission to the Universities in the next ten years. There are three factors to be considered, the increasing tendency of more boys and girls to stay on at school until the age of 18, the increase in the birthrate in the later years of the 1939–45 war and in the post-war years, and the tendency for University courses to lengthen.

It has already been stated that student numbers have been increasing by about 4,000–5,000 a year as a result of the tendency to stay on at school until 17+ years and to acquire the necessary paper qualifications for University entry; at the present time about 11 per cent. of the age group does this. The question is how high should this percentage be expected to rise. Expert opinion appears to believe that the curve may flatten off at about 16 per cent.

The effect of the increased birth-rate in the late and post-war years, which is at present passing through the schools, will reach the Universities in 1962, reaching a peak in 1966 of about 40 per cent. above the 1955 level before declining—temporarily perhaps—to about 15 per cent. above the "pre-bulge" figure.

The third factor is the tendency for University courses to lengthen, either at the undergraduate stage or by an extension into post-graduate work. This trend in recent years has been estimated to have resulted in an increase in total student numbers of about 1 per cent. per year.

While these three factors may all tend to raise student numbers in the Universities, there are two other factors which might tend to counteract them. The first is a greater degree of selectivity in the admissions policy by a number of Universities; reference has already been made to the failure and wastage rate that occurs at the present time. While the national average is a reasonably low figure, the variation between institutions is considerable and leads one to believe that there exists a "tail" which might be docked. The second factor may be the increased availability of alternative means of achieving equivalent professional status such as the new Colleges of Advanced Technology and the envisaged lengthening of courses in the Teacher Training Colleges.

Opinions differ on the combined effect of these factors. Some estimates have placed the possible University student population as high as 170,000 by the end of the 1960's. The recent announcement on future university building programmes by the Chancellor of the Exchequer in the House of Commons on February 20th stated, however, that discussions between the University Grants Committee and the Universities had been based on an increase of student numbers to about 124,000 students by the mid-1960's, with a possibility of a further increase of 10 per cent. in the second half of the decade, i.e. to about 136,000 students, some 50,000 students above the 1955 level.

The question then arises how these additional 50,000 students are to be distributed, or are going to distribute themselves, throughout the Universities. Firstly, it must again be emphasized that in a free society such as ours the student still has the right to choose his subject of study and his University. Whether or not he will find the subject or place of his first choice is another issue. Somehow or other, if buildings and staff are to be available when and where needed, attempts must be made to estimate what the demand in the various subjects is likely to be some ten years ahead; this assessment has to be made in

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the light of two considerations, (*a*) what subjects the students are likely to want, and (*b*) the needs of the State and the demand for particular types of trained men and women. These two desiderata need not conflict provided boys and girls, parents and teachers have some advice on the second and follow it. My experience leads me to believe that supply is remarkably sensitive to changes in demand, e.g. the relatively great rate of increase in technology entries during recent years and the reduction in medical entries which, in fact, ante-dated the Willink report.

From such evidence as is available in the schools and in the Universities, it appears reasonable to expect that some two-thirds of the increase will take place in Pure Science and Technology and about one-third in the Arts; there is not expected to be any material change in numbers studying Medicine, Agriculture, Forestry and Veterinary Science.

On such an assumption the relative distribution among subjects by the end of the 'sixties might be as follows:

	Per Cent. of Total Student Numbers	
	1955–56	*Late 'Sixties*
Arts	43	39
Pure Science and Technology	37	46
Medicine, Dentistry, Agriculture and Veterinary Science	20	15
	100	100

A second factor to be taken into account is the mobility of students. It has already been shown that this has tended to increase and it is expected that this trend will continue. While the distribution of certain subjects throughout the Universities, such as the Applied Sciences and certain modern languages, is becoming wider, thus lessening the need to disregard students' home Universities, other factors will more than counteract this. The inclination to study away from home, coupled, as it is hoped, with greater facilities for residence in the Universities, will make for greater readiness for students to move where vacancies exist than has been customary in the past.

The provision of residential halls in the Universities is a consideration which should be emphasized at this stage. Not only is their extension justified by the important role which they play in the students' general education, they are also necessary owing to the increasing shortage of suitable lodgings. In the large cities, full employment or the relative attractiveness of lodgers who are in residence continuously and not only for some thirty weeks in the year is reducing the supply available for students; in the smaller cities, the limit in the supply of lodgings has been reached and halls become essential if expansion is to take place in the Universities, such as Durham, Hull, Leicester, Exeter and so on.

The third and possibly most important consideration is the optimum rate of expansion and the optimum size of a University. The general opinion seems to be that there is an indefinable "organic rate of growth", to exceed which weakens the institution, though this undoubtedly varies with a number of factors such as existing size, proportion of students living in university residence, the availability of student amenities, traditions, the strength of staff-student relationships and so on. While those that are already large have reasons for not wishing to grow much larger, the smaller institutions appear to consider it undesirable to more than double their size in a decade.

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The academic and economic pressures on Universities to increase in size are strong. The proliferation of subjects is by no means confined to the Sciences; the Arts too now include numbers of subjects which were not considered essential in the Universities of the last century and many of the older disciplines show the same signs of fission as in the Sciences. Again, too, the role of the Universities in the search for new knowledge in the Arts and Sciences has also caused them to become more specialized and larger. No University can afford, if it is to fulfil its purpose, to confine itself in the long run to the teaching of a few basic subjects. Firstly, there must be close association between different fields of learning if they are to thrive; it is becoming increasingly evident that few, if any, can live in isolation from developments in continguous subjects. Secondly, teaching and research are now recognized and accepted as complementary functions of the academic staff and any institution that failed to be active in research in at least a number of fields would become a weak teaching institution. As departments and staff become increasingly more numerous, economic pressures lead to the need for an increased number of students.

On the other hand, in the prevailing pattern of university administration in this country, there is a possibility that Universities can become too large. London, Oxford and Cambridge, with their different form of internal organization, may be regarded as special cases, but there is a school of thought that maintains that most Universities in this country could not exceed seven or eight thousand students without a major change in their internal organization and a move towards the type of administration found in the larger American institutions.

The University Grants Committee was asked in November 1956 by the Chancellor of the Exchequer to advise him on the future expansion of the Universities to meet the potential increase in student numbers. In the light of numerous factors such as the above, the Committee, in consultation with each University, has established a target student number and has discussed with each University the buildings that will have to be started in the four years 1960–63. If these targets were achieved than the size distribution would be

TABLE 4

Distribution by Size of Universities in Great Britain, Excluding Oxford, Cambridge and London, in 1955–56 *and Estimated Distribution in the Late 'Sixties*

	1955–56		*Late 'Sixties*	
Full-time Student Numbers	*Number of Institutions*	*Cumulative Percentage of Total Student Numbers Per Cent.*	*Number of Institutions*	*Cumulative Perdentage of Total Student Numbers Per Cent.*
Under 500	1	0·4	1	0·3
500–999	7	11·7	—	0·3
1,000–1,499	6	26·9	2	3·4
1,500–1,999	2	35·2	1	5·2
2,000–2,999	4	55·0	11	33·5
3,000–3,999	3	74·6	1	37·4
4,000–4,999	3	100·0	1	42·2
5,000–5,999	—	—	5	71·8
6,000–6,999	—	—	4	100·0

altered as shown in the accompanying table. In 1955–56, 16 of the institutions, containing 35·2 per cent. of the total student numbers were under 2,000 in size; by the late 'sixties it is expected that there will be only 4 institutions, containing only 9·2 per cent. of the

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students, in this group; more than half the students will be in institutions of over 5,000 students.

The task of equating, say, Arts places and Science places to meet possible applications ten years ahead is no easy one. It becomes infinitely more difficult when estimates have to break down the Science expansion into the different Physical and Biological Sciences and the Technologies. Yet some effort must be made to make these calculations if the Universities' building programmes are to be realistic.

So far as the expansion by the end of the 'sixties is concerned, the assumption is being made that two-thirds of the increase will occur in the Pure Sciences and in Technology and the remaining third in the other subjects. No better basis has been put forward and it is reflected in the provisional distribution of the Government's £60 million allocated to the Universities for the four years 1960, 1961, 1962 and 1963. Of the amount to be spent on major buildings, about one-third is likely to be devoted to such accommodation as halls of residence, student unions and refectories. Of the amount to be spent on accommodation for teaching and research, 74 per cent. may be in Pure Science and Technology and 26 per cent. in Arts and other subjects, libraries and other faculty buildings. It is a partial comfort to remember that the numbers of Arts "places" are more flexible than the numbers of "places" in Science and Technology. If the swing to Science is not as marked as expected, there may be some slight over-provision of Science accommodation, but this miscalculation will not necessarily deprive any additional Arts students of a University education.

While such allocations may be made of places and capital programmes agreed on the basis of various criteria, it by no means follows that the distribution will be realized. Unfortunately places and applications for admission are not completely interchangeable. Some years ago an attempt was made to distribute State Scholars among the Universities by means of quotas, but the experiment was quickly dropped as being well nigh impracticable and academically undesirable, apart from its encroachment on the students' freedom of choice.

It has already been indicated that the problem of selection has caused the Universities some concern; while the enquiry by the Committee of Vice-Chancellors and Principals was, on the whole, comforting so far as the 1955 entry was concerned, the competition for places has increased considerably since then and is likely to intensify in the next decade. This is undoubtedly a problem for the Universities to solve themselves; freedom to select their students is one of the three most important components of the autonomy which the Universities enjoy today. It is hoped that the Universities, who are actively studying this problem, will together produce some solution which will, in the first place relieve parents, boys and girls and school teachers of some of the anxiety and difficulties which they experience in seeking places for admission. The solution must also ensure that the fullest possible use is made of the increased facilities being provided in the Universities and that uneconomic and wasteful distribution between Universities is minimized.

Apart from the more physical problems of increasing numbers and their distribution, there are also the problems of the content of university education. The dangers of over-specialization, of over-crowded curricula and of the separation of the Humanities and the Sciences are being discussed widely both inside and outside the Universities. Efforts are being made by joint discussion between the schools and the Universities to find agreement on the relationship of University entrance requirements to VIth Form education and the

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area of disagreement is by no means as wide as popular and political opinion maintains. The Universities are frequently blamed for the early specialization to which the schools are alleged to have been driven. Some Universities, and particularly some Science departments within Universities, have undoubtedly been demanding higher qualifications in specialist fields. But the tide of academic opinion seems to have turned and there are signs that some degree of liberalization is being sought.

The problem of the length and content of curricula is again one which should receive and is receiving attention in the Universities. It is undoubtedly true that the length of courses in British Universities is generally shorter than those abroad, but the length of courses must be related to the school systems in each country; entrants to the British Universities have probably done at least one year's work of university level before leaving school; again, the proportion of full-time students who complete their University course without interruption is probably higher than in most overseas countries; and finally, the proportion of students, particularly in the Sciences, who stay on after their first degrees for post-graduate instructional courses and for research in this country is high. But even if it were agreed that undergraduate courses are shorter than is ideally desirable, is it practical to consider at the present time, when the Universities are faced with an increase in numbers of possibly 40,000 in the 'sixties, any general lengthening of undergraduate courses? To do this by one year, for non-medical courses, would increase the full-time student numbers by a further 20,000 without any gain in the output of graduates. It would appear that the more feasible policy is the one which has been followed in the past—namely to retain in the Universities a proportion of the students after they have obtained their first degrees, either for instructional courses or for research, for a further period of one to three years.

The question of the content of curricula is, happily, one that is in general left for the Universities to determine, in some cases in the light of the requirements of professional bodies. That many people are not completely satisfied with the present position is indicated by the discussion and experimentation that is taking place in a number of institutions and subjects. It is true that the ferment has never reached the degree of activity that it has in, say, the United States, but it cannot be implied that nothing is happening. There are two aspects of this problem. The first is the content of specialist curricula. The volume of knowledge is increasing not only within the students' own field but in contiguous fields. Undergraduate courses become increasingly crowded as additions are made and it is only seldom that compensating abstractions are achieved. It has been said that "never has the human mind been master of so many facts and sure of so few principles" and it must be admitted that there is more than an element of truth in this accusation. But it would be unrealistic, and possibly academically undesirable, particularly where teachers are themselves continuously working at the growing points of their subjects, not to expect them to devote part of the curricula to passing on the fruits of modern research and scholarship. There is, however, need for continuous examination, and possibly cutting, of curricula by faculties if they are to be able to meet the criticism that their courses are unduly devoted to the inculcation of facts.

The second aspect of this problem is the danger of excessive specialization—in the Arts as well as in the Sciences. The foundations of a general education are, and should be, laid in the schools but building must continue during the University years. If the Universities are to turn out the graduates which the community requires of them, these

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graduates should have need of more than the specialist knowledge gained from the courses they have taken.

If time is to be released for general education, how is it to be used? Broadly, there are three possibilities; to introduce into the curriculum other subjects from a different field of study, on which the student will be examined; to provide lectures or other teaching on subjects from a different field of study without including them in the examination; and to leave the time saved to the student to use as he will. There are, of course, many arguments in favour of and against each of these approaches to the problem. But it is good that many experiments should be attempted in the light of particular subjects and circumstances. My own opinion, in so far as it is worth anything, is that, for the scientist or technologist, the most hopeful line is to introduce the student, not necessarily in his first year, to some subjects in the Humanities which are cognate to his specialist course. Social studies have, I think, an especially important role to play and they may well form the bridge with the other Humanities. On the other hand, the introduction of Science to students in the Humanities is a less tractable problem. The experiment which the University College of North Staffordshire is carrying out may provide an answer; the discussion at Cambridge may provide another. The only certainty is that thinking on the subject—and fundamental thinking—was never more necessary than today.

It would be wrong, however, to leave this subject of specialization or general education as a matter of academic and intra-curricula concern. The education of the student by the students themselves is a consideration that is often overlooked. The value of community life within a University is now universally recognized as an important function in general education (University Grants Committee, 1957*b*). A considerable part of the advantages of Oxford and Cambridge lie not only in the tutorial system but also in the bringing together of students from different backgrounds and studying different disciplines into residence in college for at least part of their academic life. It is hoped that, even in

TABLE 5

Recurrent and Non-recurrent Grants made to Universities by the Treasury on the Recommendation of the University Grants Committee in Recent Years, with Estimates for 1957–58 *to* 1961–62

Academic Year	*Recurrent* £000	*Recurrent* *Percent Increase Over Previous Year*	*Non-recurrent** £000
Average 1936–37 to 1938–39 . .	2,048	—	84
1947–48	8,942	—	2,148
1951–52	16,600	9·0	7,002
1952–53	20,000	20·5	5,746
1953–54	21,003	5·0	5,516
1954–55	24,425	16·3	6,201
1955–56	26,235	7·5	7,082
1956–57	28,253	7·6	9,134
1957–58	34,140†	20·8	12,500
1958–59	36,000†	5·4	14,000
1959–60	38,200†	6·1	15,000
1960–61	40,700†	6·6	16,600
1961–62	43,600†	7·1	18,750

* Including grants for buildings, sites, fees and equipment.
† Including estimates of the cost of salary supplementation.

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the face of so large an expansion of total student numbers, the proportion of those in residence in the Universities will have increased by the end of the 'sixties.

The third problem of expansion which must be mentioned, even if only briefly, is the question of cost. The accompanying table shows how the cost of Universities has risen in the last two quinquennia with current estimates of the grants in the present quinquennium. The sudden increases in recurrent grants in certain years require some explanation. Between 1951–52 and 1952–53 there was a change in the procedure for the payment of equipment grants; an increase was made in the recurrent grant for 1952–53 for the cost of equipment which had been previously met by non-recurrent grants and subsequently non-recurrent grants for equipment have been confined to equipping new accommodation. The exceptional increases in 1954–55 and 1957–58 were due to the revision of academic salaries which account for about half of the expenditure of the Universities.

The factors which have caused and are causing these increases in State expenditure are primarily the increase in the number of students; the increase in Science and Applied Science relative to Arts; the relative increase in research, particularly in the Sciences; the increase in the numbers of academic staff which has been proportionately greater than the increase in the number of students; the increased cost of staff per head; the increased provision of the accommodation, whether for teaching and research or for amenities such as student residence, student unions, refectories and staff clubs; and the relative stability of the Universities' other sources of income such as fees, endowments and so on.

There is firstly the capital cost per additional student. The estimate of this figure is complicated by the fact that capital expenditure on university buildings in recent years reflects not merely the increase in the number of students but also the inadequacy of university buildings in the war and immediate post-war years. If the total of all building programmes, actual and proposed, since the war is compared with the total increase in student numbers since 1938–39 and the mid-'sixties, the apparent capital cost per student place, even after adjusting for the fall in money, is only about £2,000. If allowance is made for the cost of making good obsolescence, the net cost per additional student is appreciably less than the £2,000. The figure also includes an element due to the cost of providing residence for an increasing proportion of the students.

It is worth remarking that this relatively low cost per place is achieved by expanding existing institutions as against building completely new institutions. On such little evidence as is available, the cost of these is probably at least twice as great, at least in the early stages of their development.

Estimation of recurrent costs is complicated by the fact that the cost per student varies very appreciably between faculties. An attempt to estimate these differences has been made on the basis of the 1956–57 departmental expenditures; numerous assumptions have been made on such matters as the weighting to be attributed to advanced students, or to part-time students, the amounts by which "service departments", e.g. the Pure Science departments, should be credited for their teaching of applied science students, and so on. The results are roughly as follows:

	*Estimated Average Cost per Student per Year** (£)
Arts	350
Pure Science and Technology	500
All other subjects	800

* This "cost per student" should not be taken as the cost per undergraduate, since expenditure on post-graduate work and research is included in departmental expenditures.

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On the basis of these figures and on the assumption (*a*) that price level is stable, (*b*) that academic salaries remain at their present level, (*c*) that other costs change in proportion to the change in the student numbers, (*d*) that the balance of teaching and research remains as at present, and (*e*) that the distribution of students among the faculties in the late 'sixties is as suggested earlier, then the total recurrent expenditure by the end of the 'sixties might well be £65–70 million a year compared with the present level of £46 million.

Again, on the assumption that other sources of university income remain static, except for income from fees which may be assumed to increase at the same rate as student numbers, the State's contribution to recurrent expenditure might rise to about 80 per cent. of the Universities' expenditure. If the level of capital expenditure is assumed by that time to be over £15 million a year, it will be seen that the State's commitments will have reached a very high annual figure.

All this is, of course, sheer crystal gazing, and as such is very dangerous. But it leads to the last and possibly most important problem—the relations between the State and the Universities.

In the first place, Parliament has accorded to the Universities of this country a degree of freedom that is almost without parallel in any other country where the State has an important role in financing their activities. On the other hand, the Universities themselves are very conscious of the privilege and licence which they have been given and consequently take their responsibilities very seriously. They have, as was indicated earlier, accepted the view that the Government has the right to assure itself that every field of study which it considers important and which is considered to be of University status is in fact cultivated in the University system.

The history of university development since the War justifies the Universities' claim that they have met the demands by the State for increased numbers of graduates in specified fields. They are approaching the next decade with the same determination to achieve the general increase in the output of graduates and the specific increase in scientists and technologists which the Government has indicated to be necessary. If sufficient students come forward in adequate numbers in the desired subjects and if the Universities are given the financial means, they will reach the targets. At the same time, they have developed their research activities so far as seems to be desirable. The Advisory Council on Scientific Policy, reviewing the research and development efforts of the country as a whole in the light of the financial and scientific manpower position, have stated that they consider that the proportion of the nation's scientific manpower engaged on research in the Universities seems to be a reasonable one and, in the light of competitive demands by industry and others, they do not advocate any large growth beyond that caused by the increased numbers of staff and students in the next ten years (Advisory Council on Scientific Policy, 1957, p. 9). Nor can it be claimed that these national needs have been met at the cost of lowered academic standards, either in graduate output or in research.

In return for recognizing their responsibilities to the State, the Universities claim a degree of autonomy which they maintain is essential to their success. In talking about university autonomy, it is necessary to be clear what is claimed and what is not; essentially what is claimed is freedom of thought. What has happened all too recently in some other countries, however, proves that freedom of thought requires freedom from any form of political intervention that might prejudice this right. Parliament, in its wisdom, has seen to it that university education in this country has never been the subject of political

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conflict; it has accepted the principle that in a free society the Universities should remain free and has therefore accorded to the Universities of this country a degree of freedom that is almost without parallel in any other country where the State takes a part in financing their activities.

Freedom of thought requires three ancillary freedoms. The first of these is freedom to make their own appointments to teaching posts, since otherwise political orthodoxy might become a condition of academic advancement and the pursuit of truth might be distorted.

The second is freedom from external control over what is taught, which means freedom from interference in teaching, examinations and the award of degrees.

The third is freedom from external control over admissions, so that the Universities can select those most capable of benefit from their courses, without regard to creed or colour.

The experience of the past ten years has shown that the State's needs and University autonomy have not been irreconcilable. Its continuation depends on mutual understanding of each other's needs, an understanding which it is the function of the University Grants Committee to promote. Its members are chosen for their knowledge of University or cognate affairs and are capable of representing University views to the Government as well as representing Government views to the Universities. The University Grants Committee reported, at the end of the 1947–52 quinquennium, that:

> "The prospect of its continuance seems to us to be good. The freedom which the State has left to the Universities has, we believe, fostered on their side a sense of responsibility which might have withered under any attempt at control. Moreover, the dependence of the Universities on the State is balanced by a dependence of the State on the Universities. . . . This mutual dependence is, we believe, fully realized on both sides, and has led to a sense of partnership which is full of promise for the future".

The events of the past quinquennium do not appear to have upset this happy conclusion.

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Discussion on Sir Keith Murray's Paper

Professor Bradford Hill (proposing a vote of thanks): Sir Keith's paper falls, as he himself points out, into a class for which the Society's *Journal* has been deservedly renowned—"papers and discussions which were not severely statistical but simply matters of public moment." They have dealt with such matters on broad statistical lines and often with a great deal of excellent statistical illustration. Indeed, if I were called upon to write a history of the social and economic development of this country in the nineteenth century the Society's *Journal* would be one of the first sources of information to which I would turn. It is, I believe, a matter of regret that we have so few papers of this kind offered to us nowadays, and that "descriptive statistics" should appear to be used sometimes as a term of derogation. A learned society is not so very different from a university. Its members are rightly imbued with a continuing desire to learn and to advance knowledge. But if they become too fascinated by techniques, or by methodology applied in a relatively narrow field, they may well lose, just like the over-specialized university student, the broader cultural education that should follow from contacts with differently constituted minds and from familiarity with "matters of public moment". I am glad, therefore, that Sir Keith Murrary has been persuaded to lay before us so skilfully a matter of very great importance to all educated persons to-day—the development of our universities and the workings of that quite remarkable body over which he presides, the University Grants Committee.

Working in a postgraduate institution I found particularly welcome the great importance he attaches to research, his view that "good teaching cannot often live without research; without research, teaching of the able young tends to become stale and unprofitable". For the able, more mature and not-so-young postgraduate student in a School like this the position without research would be disastrous. "It would be unrealistic", writes Sir Keith, "and possibly academically undesirable, particularly where teachers are themselves continuously working at the growing points of their subjects, not to expect them to devote part of the curricula to passing on the fruits of modern research and scholarship". I would banish his caution; I would say firmly that in the medical field all teachers should be continuously engaged in research and are confidently expected to pass on the fruits of these labours. There are, however, two subsidiary points of substance that I would make on his behalf. God forbid that this presentation should promote merely the inculcation of facts. It should, surely, convey something of the excitement of the chase, of the continual questioning of authority, of the general principles involved in research and in the specific answers, and of the outstanding and, no doubt, still great ignorance. Secondly it has been noted by astute people that some first class teachers are not really first class research workers and, equally, that not all leading research workers have the gift of conveying their thoughts to others. A great deal of modern research depends, however, upon team work and in a well-staffed university department one might hope to distribute the loads and to find that right balance between teaching and research to which Sir Keith refers. The difficulty may well lie in finding the money, for a well-staffed department for research in medicine to-day, and in science generally, is very expensive. There is no escape from that. Research is expensive not only in capital costs—in essential gadgets such as the electron microscope—but in the recurrent cost of the staff needed to maintain these gadgets—properly paid technicians and the like, as well as in the low ratio of students to academic staff of which I am glad to see Sir Keith speaks so approvingly.

In regard to this ratio, he makes a generalization which I would be happy to dispute. One of the three great merits of the low staff/student ratio is, he writes, a "marked improvement in teaching methods; undergraduates can be taught in smaller groups, seminars or discussion groups, or even given tutorials, individually or in twos and threes". He deplores the unevenness of this development and that even in two similar departments seminars and tutorials should prevail in one and large classes and lectures in the other. "One is driven to the conclusion that the limiting factor is not always a shortage of staff but the inclination of the head of the department". I do not believe one need be so reluctant to

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reach that conclusion and I do not believe that the answer to the age-old problem of how best to teach is automatically solved by replacing a large number with one who talks by a small number all of whom talk. The solution is not likely to be the same with all persons and in all circumstances. I have known teachers who could hold and inspire a class throughout an entire hour's lecture, who could not only keep them awake but awaken their imagination, teachers who would have been a complete flop at a tutorial. I have suffered the reverse. Have not we all? There is, indeed, no one answer. It may well be that lectures should be more sparingly inflicted but I do not believe that their abolition in favour of seminars and tutorials *ad infinitum* would bring us to the golden age of university teaching. We might indeed have lost on balance.

I turn finally to Sir Keith's "last and possibly most important problem—the relations between the State and the Universities". At first sight university freedom would not appear to be seriously imperilled by the fact "that the Government has the right to assure itself that every field of study which it considers important and which is considered to be of university status is in fact cultivated in the university system". It seems a trifle unlikely that the Government would detect a subject worthy of academic recognition which had escaped university notice. The dons are not that sleepy! The rub might come, perhaps, from those innocent words "which is considered to be of University status"—considered by whom? Or, still more, there is the question of the *degree* of effort which should be put into any field of study. The Government and the universities might well differ over the relative importance as subjects for study of dead languages and dead people. Perhaps, finally, to Sir Keith Murray's three ancillary freedoms one might add the freedom to publish one's thoughts.

On paper the whole situation, with its increasing dependence on Treasury money, may well appear dangerous and indeed unworkable. The fact is it *has* worked admirably and *is* working admirably; I believe we can share the author's optimism that it will continue to work admirably. For that, all university men and women in this country owe an immense debt of gratitude to the University Grants Committee and not least to its present chairman.

Mr. Frank Davies: Firstly, may I convey a message of apology from my chairman, Lord Heyworth, for his inability to be present. He was much looking forward to seconding this vote of thanks to Sir Keith Murray and I consider myself very fortunate to be given this opportunity to discharge that pleasant office.

Lord Heyworth would have spoken from the point of view of a chairman of an international business. I speak from the much humbler position of a member of the Personnel Division of that international business which has some regular contact with the universities of this country, primarily in the field of recruitment for management.

Sir Keith tells us that he no longer claims to be an academic but his comprehensive survey on the subject of universities in Great Britain can only be described as scholarly in the truest sense of the word.

The revised terms of reference have undoubtedly placed a very great responsibility on the shoulders of the University Grants Committee for the development of our university system, and if it is true that the man who pays the piper calls the tune, then as Chairman of a Committee which provides 70 per cent. of the university income to-day, Sir Keith's ear for music plays a vital role in our national affairs.

It is with this thought in mind that I venture to offer comment from the standpoint of industry about the output of university graduates. Whilst precise figures on the subject are not readily available, no one will doubt that industry is the largest employer of graduates to-day. The survey conducted in 1956 by Political and Economic Planning, on a substantial sample of about 4,000 graduates, disclosed that a little under one-third went into industry (including mining, oil, building, public utilities and manufacturing industry). Teaching accounted for a little under one-quarter, and the next largest consumer was the Civil Service with under 10 per cent. The remainder went in smaller numbers to a variety of other types of employment.

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A significant part of the management structure of British industry to-day is represented by university graduates and as the output of the universities increases in the way which Sir Keith has indicated the importance of the role of the graduate in industry will continue to grow very significantly.

The qualities possessed by these men and women are, therefore, a matter of very great moment for the future efficiency and health of our economy, and the development of the right qualities is a subject of great importance to all who are interested in the universities.

Looked at from the point of view of the industrial consumer, it is not so much a lack of knowledge of industrial administration or skill in the subjects which they have been reading, which concerns industry; it is the degree of immaturity of a proportion of young men offering themselves for employment. This immaturity seems to be most marked in that group which, for one reason or another, has not had the broadening experience of some form of National Service; the ending of National Service may, therefore, be accompanied by an intensification of this characteristic.

Recognition and discussion of this problem as between the universities and industry have not so far been particularly fruitful, in part because the frame of reference in which the problem is perceived by each side is so widely divergent. And yet if—quoting the terms of reference of the University Grants Committee—"the universities are to be fully adequate to the national needs", this is a problem area which should most certainly be explored.

It could be said that in the eighteenth century English universities, at least, were closely geared to the society from which they drew their students and to which they returned them. The older English universities of that period provided what was almost vocational education for the squire, the clergyman, the lawyer, the doctor, many politicians and some writers. Young men at university in those days cannot have been in much doubt as to what their future career was likely to be.

To-day, the situation is radically changed; and although the introduction of many new faculties has to some extent solved the problem of adapting the university to meeting the requirements of modern society, the problem of training young men who are ultimately intended for industry cannot be regarded as having yet achieved a satisfactory solution.

Sir Keith has drawn our attention to the way in which undergraduate courses become increasingly specialized and has very rightly observed that exclusive devotion to some specialized discipline will not give the student all he needs as a preparation for life. I am sure that he is right to underline the value of community life and the importance of the role played by residential halls in this general development of students.

Sir Keith has reminded us that in addition to providing sufficient graduates for the national needs, the university has another central aim—namely that of research and the thought that strikes me here is that the problem area I have mentioned may be a very proper subject for university research.

The work of discovering what qualities are required by graduates entering industry which will enable them to make the most effective contribution as managers and how these qualities may best be developed, should prove invaluable and perhaps point the way to new developments in techniques of education and training. Such research work would provide another common meeting ground for university and industry at which fruitful exchanges could take place.

Industry will surely welcome the academic researcher in its midst and I believe that the universities, for their part, would not look unfavourably on this form of research and the opportunity it would provide for discovering more about the kind of preparation required by men and women for them to take their place in the world of industrial affairs in this country at all levels of management responsibility.

There is a project under active consideration by the Leverhulme Trust to provide openings in industry for a number of young dons in the 28–32 age group for a period of two years. The aim would be to provide them with a practical insight into the approaches and techniques implicit in successful business administration, and during the period of

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secondment from their universities they would be regarded as employees of the company to which they might be attached.

The Trust has already secured the co-operation of a number of prominent industrialists, bankers, etc., who have undertaken to provide these opportunities, and each secondment will be arranged by an advisory committee to suit the needs of the particular individual.

Sir Keith has referred to the need for avoiding the dangers of specialization and the importance of ensuring that graduates possess in sufficient measure those broader human qualities required of a person who is to play a responsible role in society to-day. He stated that fundamental thinking on this subject was never more necessary than to-day and it is very reasonable to hope, therefore, that the University Grants Committee will look with favour on any work that will bring about closer collaboration of university and industry in this matter.

The vote of thanks was put to the meeting and carried unanimously.

Miss RUTH COHEN: I too should like to refer to the question of specialization and its relationship to the overloading of the curriculum.

It does seem to me that one of the serious problems of future development is the fact that—at any rate in a number of subjects and universities—the undergraduate's time-table is so full that he already has far less time than he used to have for reading and thinking out problems for himself. If we are to have less specialization, as I hope we shall, there is a very grave danger that the curriculum may get even more overloaded. I am all for allowing the student considerable time to develop interests outside his main path of study, but if he is not to be allowed on the whole to develop them on his own, with some stimulus, perhaps, from lectures and one hopes certainly some stimulus from discussion, we shall, I think, get him far too much regimented.

I would hope, therefore, that the development would be in the direction of having less time inevitably devoted to the main subject of study, keeping the formal examining to a specialized subject and allowing the student to browse about on the other subjects. Unless the student is allowed considerable freedom there is a danger of his studying a lot of things superficially and not following the direction he is most interested in because he feels he has to answer an examination and must cover the whole field; there is also, perhaps, a danger of his taking too much time from the main subject of study. I myself firmly believe that the most important thing for the majority of students is to teach them how to think, how to go deeply into a subject, and how to master anything in which they are interested. I think that on the whole this will be done better if they spend a considerable time on one subject, or one group of subjects, and see what is involved in really mastering something, than if, in their formal studies, they also have to cover a wide field.

How one is to reconcile that sort of policy with a university education which, for most people is no longer than before, it is not easy to see. I know that a number of people say that undergraduate courses in their particular subjects should be lengthened. It seems to me that we must accept the fact that this is impracticable, at any rate for the moment. I am inclined to think that for many it is also undesirable and that the solution is the one which Sir Keith Murray has also mentioned, that rather more of the better students should pursue postgraduate courses of study. In a number of fields, they now start on research when they have taken their degree. If we are to have the curriculum lightened, I think the ones who are going deeply into any subject will have to have a year, or possibly two years, of postgraduate study mastering their own subject. This will be necessary for only a few selected students and will be cheaper than a general lengthening of the course. Unless one does something of this sort, if one widens the curriculum one will be forcing more specialization back on the schools, and that, surely, will be the most undesirable course of all.

Dr. J. F. FOSTER: I should like to add my thanks to Sir Keith Murray for his masterly paper, and to congratulate the Society on its wisdom in arranging for a discussion on a topic of this sort.

We hear a good deal about the student population, actual and future, of this country,

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but in my opinion the thread that must run through any discussion of this subject is the principle of selection which is applied here. Sir Keith referred to the failure rate in another country and to the comparison that is frequently made between the proportion of students in the British population and in the American population. One has to bear in mind that such comparisons seem to be very largely invalidated by the fact that here we operate a system of selecting students, whereas in most other countries the doors of universities are open to all students who can satisfy the minimum entrance requirements. If they fail in their first year examinations they are ruthlessly thrown out or forced to discontinue their courses. Most of these students would not gain admission here, and the two sets of figures, I suggest, do not really bear comparison.

It is said, perhaps with some exaggeration, that the hardest test faced by a student here is that of securing his first admission to a university. Once he has been chosen, he can expect to be well looked after, well taught, and given every opportunity to complete his degree, so that the chances of failure, if not negligible, are at least minimal. In the United States, France, Australia, and indeed the principal academic countries of the world outside the United Kingdom, students fail more frequently because they are taken without much or any preliminary selection. Which is the right plan is a matter of opinion, but the United Kingdom system of which Sir Keith has spoken admits of very much economy of effort and eliminates a great deal of waste from the point of view of the teaching staff and also of the financial resources of the student and his parents.

The Vice-Chancellors' Committee has recently promoted a survey of the number of applications that are made for admission to universities. Sir Keith Murray's statement that there are some sixty or seventy applications for entry to a certain college for every one place can be repeated in other instances, but it must be remembered that many of those same applicants will also have applied to six or seven, or ten or fifteen, other institutions, and that to some extent these figures cancel out. The survey carried out by Professor Glass of which the results were published in the report, *Applications for Admission to Universities* (R. K. Kelsall), suggests that out of every five qualified applicants for university admission in a given year four eventually secure places at one university or another, not necessarily the one of their first choice.

I should like to ask one question in regard to Sir Keith's account of possible expansion of university facilities, to which we are all looking forward in the next few years. He spoke about the growth of existing institutions, but I do not think he said anything about the promotion of new ones. Ten years ago, there were in Britain some sixteen universities and five university colleges. To-day those five colleges have all become universities and one new university college has come into existence. Although the number of universities of full standing has grown in this way, the effective number of academic centres and teaching units remains substantially the same; only one new institution has come into existence during the decade, though the creation of another is adumbrated by reports recently made in the press. Does Sir Keith contemplate that the next ten years will see more universities and university colleges coming into existence to meet this demand that he has so ably described to us?

Mr. HUTTON: As a user of statistics, I welcome this paper on a rather more pedestrian level. I hope it will encourage some, if not all, of the universities to put information about students on individual record cards. It seems a great pity, when the cost of students is estimated at about £500 a year each (and the total cost to the country must exceed that), that we cannot afford to spend a shilling or so on maintaining an index which would enable all our questions to be answered. It would pay for itself several times over by enabling us to trace wastage, the proportion of overseas students, the question of double counting of people taking degrees and diplomas, even the double counting of people admitted successively to first degree courses in more than one university, and all the things which have been worrying us.

The most interesting part of the paper I found in the discussion about the future. I have never understood why it was regarded as proper to assume that we should reach a

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60 per cent. increase in the proportion staying on at school and then stop there. Indeed, unless industry greatly expands its intake of sixth formers coming straight from school, the figures so far published for the expansion of the universities, the advanced courses in the technical colleges and the teacher training colleges combined, will fall so far short of the probable output of educated people in the schools that it might well have an inhibiting effect upon the growth of the sixth forms upon which the whole of this operation depends; one must watch that very carefully.

In this connection, I was disappointed to see that Sir Keith Murray gave currency to this myth (if I am not treading on Scottish corns) that a higher proportion of the 17-year-olds stay on at school in Scotland than in England and Wales.* Such is not the case. The latest figures, for January, 1957, were that 9 per cent. of the live 17-year-olds are in school in England and Wales. Compare that with only 8·3 per cent. in Scotland; and even this Scottish figure is inflated by the inclusion of people in approved schools and independent schools not recognized as efficient, which we in England and Wales still omit.

Of course, this myth has an historic source but it has been buttressed in recent years by the Scottish device of taking the census three to six months later in the academic year than is done in England. That stopped in 1951 and the figures are now comparable. But the figures given in the *Annual Abstract of Statistics* show the catastrophe which appeared to hit Scottish education when Scotland changed from the July basis to the same basis as England and Wales. It made one seriously question the returns at the time.

The date by which the age is taken has an influence also on pre- and post-war comparisons. We, too, have sinned. We used to take our returns in March and now take them in January, and I think for that reason the increase in the proportion staying on at school is even greater than this paper suggests.

I do not recognize, as perhaps I should, some of the figures which are given of the 17-year-olds in school. The difficulty is that there are some independent schools not recognized as efficient; and one must take a single age-group, the 17-year-olds, in order not to run into trouble with the 18-year-olds who are affected by the impact of National Service and other influences.

Finally—perhaps it is an improper suggestion—I would plead for some mention of those hard-working people who get their degrees the hard way—that is, the external students, and the internal students of the Polytechnics. I realize that Sir Keith Murray limited himself severely to the University Grants Committee grant-aided institutions, but it is not generally recognized that part-time students at the technical colleges count for as much as one-fifth of the technology degrees and I think nearly one-sixth of the science degrees in this country are omitted from the University Grants Committee figures although they have some influence on the figures quoted here.

Professor PEARSON: Sir Keith Murray has given us a most interesting account of the University Grants Committee's broad strategy of university planning as seen against the changing character of university populations. I think we can admire his courage in coming to speak at the heart of the largest university in this country, where a number of his audience are only freshly come from meetings at which agonizing decisions have had to be taken on what existing scientific activities must be cut down next session from lack of funds!

However, I do not want to dwell on this aspect of the matter, but on another question which concerns this Society rather closely. On p. 402 of his paper Sir Keith has written:

> "My experience leads me to believe that supply is remarkably sensitive to changes in demand"

and he then refers to the entries in technology and medicine, the first increasing, the second falling in response to demand. Unfortunately this situation does not seem to hold in the case of statisticians and it is perhaps interesting to consider a little why this is so. I

* The author has since amended this statement.

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speak as a member of the committee appointed by the Society's Council to look into this question of the demand for and supply of statisticians.

As to the unsatisfied demand, of this there is very little doubt. The Clapham Report of 1946 (to which Sir Keith has referred) called attention to this demand, but after 12 years we can only say that the position is still most unsatisfactory. It is only necessary to hear our President speak with despair of the impossibility of building up the Statistician class in the Civil Service, or to note the continuing struggle to fill university teaching posts.

Very briefly, one main point of difficulty seems to be this. The techniques of present day statistical methods are becoming increasingly mathematical, and the source of supply which the university teachers of statistics must draw upon consists of the output from the schools of mathematically minded boys and girls. This output is limited and there are at present very heavy demands on it.

There are between 600 and 700 entrants annually to mathematics courses at the universities of England, Wales, Scotland and Northern Ireland (Kelsall Report on the 1955 entry). Of these only about 400–450 (U.G.C. Report for 1956) are graduating in mathematics, a very considerable number transferring to other courses—e.g. to physics or engineering, after a first year of mathematics. In 1955 about 270 applicants failed to obtain university entry in mathematics, but it was estimated that some 200 of these had not suitable qualifications and it is certain that they would not have been very suitable material from which to make statisticians.

Next, as to the demand for mathematicians. In the 1956 *Report on Scientific and Engineering Manpower in Great Britain*, issued by the Ministry of Labour, it was estimated that in the three years 1956–59 the number of qualified mathematicians in this country should, if possible, be increased from the 1956 figure of about 11,500 to some 13,900, i.e. by 2,400* or 800 a year. This figure does not include replacement of wastage—from retirement and other causes—which might well be 400 a year.

This seems to mean that the Advisory Council on Scientific Policy estimates that at the moment an annual supply of some 1,200 qualified mathematicians is needed—a figure far in excess of the university output. Clearly as statisticians, we are trying to attract mathematically qualified students from a limited pool in face of strong competition.

At the moment we can only try to draw off from other fields of employment a larger share of this too small supply; we believe that a good deal can be done by making the schools more aware that there are interesting and varied careers in the statistical field for the boy or girl of mathematical bent. In the long run, however, something must be done to increase the supply of mathematicians from the schools. The solution will not be easy to find. There is the same problem in the United States. Maybe the fault goes back to quite early teaching of mathematics. The subject is made to appear too hard and uninteresting.

This is hardly a problem which any direct action of the U.G.C. can help to solve, but I have referred to it because it seems to me that there is a danger of overlooking what lies underneath when speaking broadly of supply and demand in terms of overall totals. Evidence on the way in which an increasing number of places is being found at the universities for the broad class "scientists and technologists", tells us very little about the prospects for the sub-class of "statisticians" within the class of "mathematicians".

Let me make my point with one further illustration. At the moment there is great difficulty in filling the existing university posts for lecturers or assistant lecturers in statistics. To maintain the supply, it is essential for the universities to retain some of their best students for two or three years' research training; this can only be done if grants are available at the right moment and the right place. Suitable candidates for research training occur here and there with the fluctuations to be expected when dealing with small random numbers. Is the machinery of the D.S.I.R., tuned to cope with an overall expectation for science and technology, sufficiently flexible to deal with a type of local variation

* It was estimated that about two-thirds of these were required to increase the complement of r matical teachers.

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between good and bad years and one university department and another, which I would be inclined to guess has a variance even greater than its mean?

Mr. GORMAN: I wish to touch briefly on two of the many interesting points raised by Sir Keith Murray: the number of students we are likely to have in the future, and the method of recruiting the staff required.

Let us over-simplify grossly and assume that in choosing what students should be admitted to the universities, we rely implicitly on a single criterion and that is the intelligence quotient. Such over-simplifications are embarrassing to make, but sometimes lead to useful conclusions.

An I.Q. of 122 has been mentioned as probably representing the border-line between students who are and are not at present admitted into the universities. This is, I believe, about $1\frac{1}{2}$ standard deviations above the mean of a normal distribution. A small range of 1 or 2 points below this limit would, therefore, include quite a large number of people compared with the number above the limit. I doubt if many people would claim that our present admissions procedures are so accurate that they have not a margin of error at least as great as that represented by 1 or 2 points in an intelligence quotient. But even if this were not so, it seems clear that a reduction of our standards by such an amount would not be a notable reduction and yet it would lead to a fairly large increase in the number of students. All this suggests that a small change in our methods of admitting students might, in the long run, lead to very large changes in the size of the university population and that this is so whether the small change takes the form of a reduction in standards or the use of some more accurate admissions procedures. I cannot but feel that those 5,000 boys and 5,000 girls quoted by Sir Keith Murray cannot really have much basis in reality at least from the long run point of view.

To turn to my second point. Since I have already said that it is difficult to have any idea of the probable future size of the universities (though my own feeling is that they ought to increase in size rather rapidly), it is perhaps rather foolhardy to worry about the number of staff required. Nevertheless, we may run into a pressing problem there, especially if our universities are to expand. American universities also are expanding at the present time, and their "bulge" problem is of quite a different kind from ours; not one that comes to a peak and falls off again. The size of American universities, both in the number of students and of staff, is of a completely different order of magnitude from the British.

We know that the extent to which young statisticians have been leaving English universities and going to America is influenced by the better salaries and better working conditions; the same is true, I think, of mathematics and many other disciplines; and I am sure that there will be a major loss of university staffs from this country in the next ten years unless a very considerable effort is made to stop it.

I may be wrong, but, if I am correct, it is a thing we have to start thinking about reasonably soon.

Sir KEITH MURRAY: I should like to thank you very much for the kind remarks you have made about this paper. Apart from writing the paper and therefore having to do some thinking, I have benefited greatly by the discussion.

The remarks of Mr. Hutton illustrate the kind of advantage which I have got from coming here tonight—how valuable it is for a layman like myself to produce, very timorously, some statistics and have them criticized, and very rightly criticized, by members of a Society such as this.

The following communication was received from Dr. Idenburg, Director of the Netherlands Central Statistical Office and Professor of Education at Amsterdam University.

I have made a comparison between the British and Dutch figures.

Percentages of 17-age group in the Netherlands remaining at school to 17 years or over: 1951 18 per cent., 1956 19 per cent. Only a proportion of them are qualified for admission

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to the University. Pupils finishing their study with this qualification: 1938 3·8 per cent. and 1957 5·2 per cent of the age-group 18–20.

Before the war the proportion which reached the University was 1 in 56 of the age-group 18–22, now it is similar to that of Great Britain, 1 in 30.

The number of scholarships etc. is with us much lower than in Great Britain. We know only the state scholarships which amount to 18·1 per cent. of the students.

Numbers of Students in Different Faculties

Faculty (Adapted to British Terminology)	1938–39	1957–58*	*In % of the Total* 1938–39	1957–58
Arts	4,839	14,455	38·4	44·6
Pure science	1,537	4,797	12·2	14·8
Technology	1,911	5,965	15·2	18·4
Medicine and dentistry	3,719	5,873	29·5	18·1
Agriculture and forestry	462	869	3·7	2·7
Veterinary science	124	426	1·0	1·3
Total	12,592	32,385	100	100

* April, 1958.

As you see, arts, agriculture and veterinary science do not differ very much from the British figures. Medicine and dentistry is somewhat higher with us. Pure science is much lower in Holland, technology much higher; together they come to 37·1 per cent. in Great Britain and to 33·2 per cent. in the Netherlands.

Changes in the Proportions of First-year Students in Arts to the Total Number of First-year Students

Year	*Total Number First-year Students*	*Number First-year Students "Arts"*	*In % of the Total*
1938–39	2,644	1,207	45·7
1956–57	5,360	2,456	45·8

Great Britain is diminishing to what is the traditional figure in our country.

Sir Keith subsequently replied in writing as follows:

I am most grateful to those who have taken part in the discussion and for their generous reception of the paper. I am particularly grateful to those, such as Mr. Hutton and Dr. Foster, who have pointed out factual mistakes, some of which I have been able to correct in the proofs.

Professor Bradford Hill has rightly drawn attention to the fact that all researchers may not be good teachers while some teachers may not be good researchers. I would, of course, accept this statement but I still believe that most academic staff members should combine in themselves the two functions. He later asked who should be judges of what new subjects are of "university status". In my view, this assessment should be left to each university; this is a facet of my second ancillary freedom—freedom from external control over what is taught.

Mr. Davies has stressed the immaturity of graduates who take up positions in industry. He is right in some respects but there is a danger in comparing recent graduates with those who came through the universities in the immediate post-war years. My own view is that contacts between industry and universities have become closer in the last ten years than ever before, a development which is to be welcomed. Support for halls of residence is one of the practical ways in which industry is helping to ensure that students receive

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the maximum benefit from their time at the university and acquire some of that maturity for which industry is looking.

Dr. Foster raised the question of the founding of new institutions in the next ten years. So far as the next five years are concerned, the University Grants Committee has recommended the establishment of one new institution, the University College of Sussex; it believes that, to meet the immediate expansion, it is not only both quicker and cheaper to expand existing institutions but it is also academically desirable that the smaller and middle-sized universities should increase their numbers.

Professor Pearson questioned whether supply did in fact respond to demand, particularly in the case of statisticians. The question of why mathematics is less attractive to schoolboys and girls is a complex and a very fundamental educational issue which might well be the subject of a separate discussion. I would, however, suggest that publicity and propaganda, such as has been afforded in technology and in medicine, are necessary if unfulfilled demands are to be met.

Mr. Gorman's question of staff recruitment is in the long run a vital one. I personally believe that this will be solved, provided the Universities are enabled to continue to compete with other employers in the salaries which they pay and to provide the facilities which attract so many to academic work. It would, I think, be quite wrong not to plan for the expansion and to increase the universities' accommodation for fear that we cannot ultimately solve the staff problem.

As a result of the ballot taken during the meeting the candidates named below were elected Fellows of the Society:

Isabella Nimmo Campbell
Jan Salomon Cramer
David John Kirby
Ian Stewart Lloyd
Timothy Murphy
Abdel Monein Mursi
Vrudhula Krishna Murthy
Riley Anthony Winton Rudd
Noel Spencer Shepherd
Deryck Reginald Spackman
Keith Trevor Littler

Corporate representative

John Henry Stanley Kent *representing* Hulton Press Ltd.

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Estimates of the Total Miles Run by Road Vehicles in Great Britain in 1952 and 1956

By K. N. Chandler and J. C. Tanner

Road Research Laboratory, Department of Scientific and Industrial Research

Introduction

This paper sets out five methods of calculating the total miles travelled by the various classes of motor vehicle in 1952 in Great Britain. These methods rely in part on the same data and are therefore not all quite independent. There is considerable uncertainty about the reliability of some of the data and the resulting estimates vary quite widely, thus emphasizing the caution that is necessary when using them. The year 1952 was chosen because some of the data were not available for a more recent year, but the average results for that year are later scaled up to provide estimates for 1956. Estimates are also made of the mileages run by pedal cycles. Information of this sort is required for a number of purposes, such as economic analyses, international comparisons and studies of accident rates.

Sources of Data

The sources of data used in the methods are:

(*a*) Counts of traffic made by the Road Research Laboratory and others (mostly given in unpublished reports available on application to the Director of Road Research).

(*b*) The Public Road Passenger Transport Statistics (Great Britain) for 1952 (Ministry of Transport and Civil Aviation, 1954).

(*c*) The Annual Report and Accounts of the British Transport Commission for 1952 (British Transport Commission, 1953).

(*d*) The results of a sample enquiry on goods vehicles for one week in 1952 (Glover and Miller, 1954).

(*e*) Statistics relating to the consumption of petrol and diesel fuel (derv) in the United Kingdom, for 1952 (Petroleum Information Bureau, 1954).

(*f*) The fuel consumption of vehicles.

(*g*) Numbers of licences current for various classes of road vehicle in Great Britain and Northern Ireland (Ministry of Transport, 1953; British Road Federation, 1953; Ministry of Transport and Civil Aviation, 1957).

The first source features, to a greater or lesser extent, in all the methods. Use is made of it in Appendix A of this paper where the percentage composition of traffic flow in 1952 is estimated. Results of traffic counts are also used to make allowances for trends, and to estimate the proportions of traffic at week-ends and on unclassified roads. In addition, they form the basis of the last two methods. These traffic counts are in some ways unsatisfactory for the present purposes—most of them, for instance, only cover the more important roads—but their use is unavoidable.

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Counts made at a random sample of points on the road system will provide satisfactory comparisons of numbers of vehicle-miles run in different periods and by the various classes of vehicle. Also, if the sample of points is sufficiently large, then the total number of vehicle-miles run on the road system in any period can be estimated by the average of the flows at the points during this period multiplied by the total length of roads in the system in miles.

The passenger transport statistics (Ministry of Transport and Civil Aviation, 1954 and British Transport Commission, 1953) are compiled from statutory returns made by all operators.

The enquiry on goods vehicles by Glover and Miller (1954) obtained data concerning 7,243 vehicles for the week September 22nd–28th, 1952, in which, as far as can be judged, goods traffic should have been about average. The standard errors of various statistics for that week derived from the sample can be estimated and are not more than a few per cent. The estimates for the whole year are less accurate. The survey did not cover government or military vehicles or farmers' goods vehicles.

Statistics relating to the consumption and refinery production of petroleum (Petroleum Information Bureau, 1954) are total figures for deliveries of the various petroleum products to the various types of user in the United Kingdom. There may be errors—although probably very small—owing to petrol (including motor benzole) and diesel fuel being diverted to other uses. Loss due to evaporation is taken to be negligibly small. An approximate correction is made for the consumption in Northern Ireland which is thought to be reasonably accurate compared with other uncertainties.

The data on the average fuel consumption of vehicles of various types and sizes were supplied by the Vehicles Section of the Road Research Laboratory and were given as only very rough estimates. It is found, however, that they agree fairly well with other data.

The analysis covers all types of motor vehicle, although in parts of the analysis the mileage run off the public road system is not included. Where reference is made to types of vehicle (motor cycles, cars and taxis, public service vehicles and goods vehicles), the distinction between cars and goods is mainly on the basis of usage rather than type of excise licence. "Goods" includes various miscellaneous groups of vehicles, including a large proportion of government and military vehicles, and also utilities with carriers' licences (even if with a private car excise licence).

For convenience, more digits are retained throughout the calculations than the inherent accuracy of the data justifies.

Method I

The principle of this method is to use published mileage data for goods vehicles and public service vehicles and to scale these up to the total in proportion to traffic flow data.

The total miles travelled in Great Britain in 1952 by all public service vehicles (i.e. by buses, coaches, trolleybuses and trams) is given by the Ministry of Transport and Civil Aviation (1954) as 2,530 millions. This figure will be regarded as accurate.

An estimate of 10,969 millions is given by Glover and Miller (1954) for the total miles travelled in 1952 by all goods vehicles. This figure is based on a sample enquiry concerning 7,243 vehicles. While this figure may not be as accurate as that for miles travelled by public service vehicles it is probably more accurate than the estimates of vehicle-miles

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which will be calculated in this paper. This figure does not include government-owned (including military) goods-type vehicles or farmers' goods vehicles. From other sources it is estimated that in 1952 these vehicles ran 600 million miles and also that government-owned buses and coaches ran 10 million miles. Thus the total vehicle-miles for goods vehicles was 11,569 millions, and 2,540 millions for vehicles of the public service type.

In Appendix A it is estimated that public service vehicles and goods vehicles constituted respectively 8·24 per cent. and 31·38 per cent. of all traffic in 1952 and the calculation of these two percentages is independent of the data just given. The ratio 11,569 : 2,540 is 4·555 to 1 and the ratio 31·38 : 8·24 is 3·808 to 1. If all the figures were accurate it would have been expected that these ratios would be the same, and their difference gives a measure of the errors involved in this method of calculating the total vehicle-miles.

To estimate the total motor-vehicle-miles from these data, it is observed that public service vehicles and goods vehicles travelled $2,540 + 11,569 = 14,109$ million miles in 1952 and constituted $8·24 + 31·38 = 39·62$ per cent. of the total mechanically-propelled traffic in that year. An estimate of the total miles covered by all mechanically-propelled vehicles in 1952 is, therefore, $14,109 \times 100/39·62 = 35·6 \times 10^9$ miles.

Method II

This method estimates total vehicle-miles from figures for motor-fuel consumption, taking into account the figures for the mileages of public service vehicles and goods vehicles used in method I. The method is not very reliable because it depends critically on estimates of the average miles per gallon for various classes of vehicle, but it was devised so as to make the least possible use of estimates of the composition of traffic. It uses only the flow of cars and taxis relative to that of motor cycles, and this is an important improvement on a full use of flow data because errors in this ratio have relatively little effect.

A check on the method is to apply it first to diesel fuel because this is only used in public service and goods vehicles, and the Ministry of Transport and Civil Aviation (1954), the British Transport Commission (1953) and Glover and Miller (1954) provide relatively reliable estimates of the mileages run on diesel fuel by these classes of vehicle. It is shown in Appendix B that the total fuel required to cover these mileages, on the basis of the figures used for miles per gallon, is 85 per cent. of the diesel fuel actually supplied. This is a satisfactory check on the method, considering the difficulty of estimating the figures for miles per gallon.

The vehicle-miles for public service and goods vehicles running on petrol are known fairly reliably and they can be used to estimate the amount of petrol such vehicles use. The remainder of the petrol used, except for what is assumed to be a trivial amount, is used by cars, taxis and motor cycles. The numbers of these vehicles of the various engine sizes or horse powers are known, as well as the relative flows of cars and of motor cycles. Some guess has to be made about the relative miles per vehicle per year of the various sizes of car, and similarly for motor cycles, but this does not greatly affect the results. The calculations for this method are set out in Appendix B, where it is estimated that motor cycles (including motor-assisted pedal cycles, combinations and tricycles) accounted for $3·9 \times 10^9$ miles in 1952, and cars and taxis for $24·6 \times 10^9$ miles. The total for all motor vehicles is then estimated as $42·6 \times 10^9$ miles.

This is considerably higher than the result given by method I. Possible reasons for

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this include: (i) appreciable diversion of petrol from road transport; (ii) persistent underestimates of fuel per vehicle-mile for the various classes of vehicle; and (iii) error in the estimate of the composition of traffic obtained in Appendix A.

It may be observed that if either (or both) of (i) and (ii) applied also to diesel fuel it would go some way towards eliminating the 15 per cent. unexplained consumption.

Method III

This method makes use of the total fuel consumed, the composition of traffic, and the average fuel consumption of each category of vehicle. The principle may be expressed as follows:

Total vehicle-miles = Total fuel consumed/Average fuel per mile.

The average consumption must be calculated in terms of gallons per mile, giving appropriate weight to the various classes of vehicle. The calculation is as follows:

Type of Vehicle	*p = Per cent. of Motor-Vehicle-Miles (Appendix A)*	*g = Average Gal. per Mile (Appendix C)*	*p × g*
Motor cycles	8·20	0·0133	0·109
Cars and taxis	52·17	0·0360	1·878
Public service vehicles . . .	8·24	0·0849	0·700
Goods vehicles	31·38	0·0662	2·076
	99·99		4·763

Thus the average gallons per mile is 0·04763 so that the average miles per gallon is 21·00. The total fuel consumption in 1952 is given by the Petroleum Information Bureau (1954) and this, after allowing 2 per cent. for Northern Ireland, leads to an estimated consumption in Great Britain of 1,962 million gallons. The estimate of total motor-vehicle-miles is therefore $41 \cdot 2 \times 10^9$.

Method IV

This method is based on some results of a sample survey of the distribution of traffic in three counties and one county borough, described by Tanner (1957). It was found that for these four areas, the total vehicle-miles per average weekday in 1954 was closely related to the populations in their urban and rural parts. For motor vehicles as a whole, for instance, the amount of travel could be expressed as $1\frac{3}{4}$ miles per urban inhabitant per weekday, plus $6\frac{3}{4}$ miles per rural inhabitant. Applying the relationships to the population of Great Britain as a whole, it was estimated that in 1954 the average weekday vehicle-miles of the various classes of vehicle were:

	Million Miles
Motor cycles	9·834
Cars and taxis	72·270
Public service vehicles	7·634
Goods vehicles	45·614
Pedal cycles	38·914

To convert these into annual totals they are multiplied by 365 and by factors, derived from several sources, to allow for week-ends. The figures so obtained are then reduced

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to the 1952 level by means of the trends given by Macnaughton-Smith and Tanner (1956). These calculations are as follows:

	Correction for Week-ends (7-day Average ÷ 5-day Average)	*Vehicle-miles, 1954* $10^6 \times$	*Ratio* 1952/1954	*Vehicle-miles, 1952* $10^6 \times$
Motor cycles . . .	1·137	4,081	0·874	3,567
Cars and taxis . . .	1·086	28,648	0·824	23,606
Public service vehicles . .	1·040	2,898	1·055	3,057
Goods vehicles . . .	0·819	13,634	0·928	12,652
All motor vehicles . .	—	—	—	42,882
Pedal cycles . . .	0·920	13,067	1·218	15,916

Method V

The basis of this estimate is the continuous census that has been operating since the beginning of 1956 at 50 points on the classified roads of Great Britain. This census has been described in general terms by Tanner (1957). The estimate of the total motor-vehicle-miles on the classified roads of Great Britain in 1956 is $45{\cdot}082 \times 10^9$. This may be scaled down to the level of 1952 by means of trends assessed from the results of traffic counts. (See Macnaughton-Smith and Tanner, 1956). In that paper figures up to 1955 only are given.) This gives 0·729 for the ratio of the year 1952 to the year 1956, which leads to $32{\cdot}86 \times 10^9$ for the estimate of motor-vehicle-miles on classified roads in 1952. From the sample survey, mentioned earlier (Tanner, 1957), it is estimated that vehicle-mileage on unclassified roads is 21·88 per cent. of that on classified roads. Increasing the estimates by this amount gives

$$\text{Motor-vehicle-miles, } 1952 = 40{\cdot}06 \times 10^9 \text{ miles}$$
$$1956 = 54{\cdot}95 \times 10^9 \text{ miles.}$$

Some measure of the accuracy of these estimates can be obtained from the variation in flow between the 50 points. The coefficient of variation is of the order of 20 per cent., to which must be added further components, thought to be relatively small, for errors in the scaling processes. Thus the estimate is of very limited value.

Discussion

The five estimates of total miles run in Great Britain in 1952 by mechanically-propelled vehicles are as follows:

	10^9 *Miles*
Method I.—Multiplying up the public service and goods vehicle-miles by the ratio of total vehicle flow to that of public service and goods vehicles . . .	35·6
Method II.—Estimating petrol available for cars, taxis and motor cycles after subtracting estimated consumption by public service and goods vehicles . .	42·6
Method III.—Allocating total fuel supplies to all classes of vehicle in proportion to their flow and their consumption per mile	41·2
Method IV.—Extension to the whole country of a relation between traffic flow and population found to hold in three counties and one county borough . .	42·9
Method V.—Flow figures at 50 points which were carefully selected to form a stratified random sample of classified roads; the flow being then adjusted for unclassified roads	40·1

The final results agree rather more closely than the authors expected in view of the errors to which the various methods are subject.

Methods I and III rely mainly on Appendix A, which estimates the proportions of traffic in the various classes of vehicle on all roads. If this could be done accurately

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method I would probably be the best method. Appendix A estimates that public service vehicles and goods vehicles constituted 39·62 per cent. of all mechanically-propelled traffic. For method I to produce the same value as the average of the other methods ($42{\cdot}0 \times 10^9$ miles) this percentage would have to be 33·5 per cent. It is difficult to believe that Appendix A is out by as much as this.

Methods II and III depend on estimates of fuel per vehicle-mile for various classes of vehicle. These cannot be regarded as other than very approximate. These methods also depend on the assumption that all fuel delivered for consumption is actually used on public roads in the vehicles that have been considered. For diesel fuel it has been seen that there is a gap of 15 per cent. of the total deliveries and this may represent losses, diversion to other uses, or harder use of diesel-engine public service vehicles than of comparable petrol-engine vehicles. The first two suggestions apply also to petrol; the third applies in reverse to petrol and any such tendency would lower the consumption of petrol by public service vehicles and goods vehicles and so leave more for cars and motor cycles than has been estimated. This would increase the estimate by method II.

Fairly large errors can arise in method II because, in assigning the residual petrol to cars, taxis and motor cycles (which have relatively low consumptions per mile) it makes the overall vehicle-miles figure rather sensitive to errors in the estimate of goods and public service vehicle consumption. Because these calculations do not allow for losses and diversions of fuel it is not surprising that it gives a large estimate of vehicle-miles.

Method III is also expected to give a high estimate as it makes no allowance for losses of fuel. However, it uses the total fuel consumption in a manner which avoids the unbalance of method II: it may therefore be rather better than method II. Estimates by methods II and III would be expected on internal considerations to be correct to about 20 per cent.

Method IV is subject to sources of error which, although not measurable, are thought to be fairly large. It is very largely independent of the other four methods, since it makes no use of fuel data, published mileages of goods and public service vehicles, or the composition of traffic derived in Appendix A.

Method V is also independent of the data used by the other methods except that in order to include unclassified roads it uses the same data as that from which the overall composition of traffic is computed in Appendix A. However, its coefficient of variation of about 20 per cent. is rather large.

Method V is based on records of automatic counters which, strictly, record half the number of axles passing, rather than the number of vehicles. This has the effect of overestimating the true flow by not more than 2 or 3 per cent., due to multi-axled vehicles, but on the other hand, light motor cycles may not always record.

The dependence of each method on the various items of data is tabulated in Appendix D.

Combined Estimates for 1952

To obtain the best combination of the various estimates it is most satisfactory to deal separately with each class of vehicle (motor cycles, cars, public service, goods). In the case of methods II and IV, the vehicle-miles for each class were obtained in the course of the calculations; for method I, they may be obtained by obvious extensions of the method; for methods III and V the total may be split up in accordance with Appendix A. The

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results are set out in Table 1, which also gives figures for pedal cycles. The latter, except for method IV, were obtained from the corresponding motor-vehicle-miles and the composition in Appendix A.

TABLE 1

Estimates of Mileages Run by Different Classes of Vehicle in 1952
(10^9 *Miles*)

Class of Vehicle	*Method* I	II	III	IV	V	*Combined Estimate*
Motor cycles	2·9	3·9	3·4	3·6	3·3	3·4
Cars and taxis	18·6	24·6	21·4	23·6	20·9	21·4
P.s.v's	2·5	2·5	3·4	3·1	3·3	2·5
Goods	11·6	11·6	12·9	12·7	12·6	12·0
Total	35·6	42·6	41·2	42·9	40·1	39·3
Pedal cycles	12·6	15·1	14·6	15·9	14·2	14·2

Method II is expected to be too high for motor cycles and cars; method III may be slightly too high generally. Methods I and II use a fairly reliable estimate for goods vehicles but methods III, IV and V give higher estimates. The figure for public service vehicles used in methods I and II is almost the same as that which is published by the Ministry of Transport and Civil Aviation and will be taken as accurate. The best estimates for each of the other classes, given in the last column of Table 1, are derived from those given by the five methods by giving due weight to their intrinsic accuracy and interdependence. The figure for all motor vehicles is the total of those for the separate classes.

Thus the best estimate for total motor-vehicle-miles in 1952 is $39 \cdot 3 \times 10^9$. The authors are fairly confident that the actual total lies within the range 35 to 44×10^9.

Extension of Estimates to 1956

From figures for the trend in the flow of each class of vehicle between 1952 and 1956 (calculated in the same way as the figures given by Macnaughton-Smith and Tanner (1956), which only go up to 1955), and from the combined estimates given in Table 1, it is possible to estimate the vehicle-miles of the various classes in 1956. Thus Table 2 is obtained.

TABLE 2

Estimate of Miles Run by Different Classes of Vehicle in 1956

Class of Vehicle	*Vehicle-miles*, 1952. (10^9 ×)	1956 *as a Proportion of* 1952. (*Appendix A*)	*Vehicle-miles*, 1956. (10^9 ×)	*Range Within Which True Value Most Probably Lies.* (10^9 *miles* ×)
Motor cycles	3·4	1·329	4·5	±1
Cars and taxis	21·4	1·435	30·7	±3
P.s.v's	2·5	0·925	2·3	±0·1
Goods	12·0	1·180	14·2	±2
Total	39·3	—	51·7	±5
Pedal cycles	14·2	0·746	10·6	±4

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The last column gives limits on either side of the estimate in the previous column between which the authors are reasonably confident that the correct value lies. These limits are guesses rather than the result of any calculations; they are intended to be 95 per cent. confidence limits.

The figures for 1956 would have been slightly higher if it had not been for the fuel shortage and rationing during the last quarter of the year. It is estimated, for instance, that the total motor-vehicle-miles would have been about 3 per cent. higher.

Mileage per Vehicle

So far the analysis has dealt mainly with aggregates for the various classes of vehicle, but the results can also be expressed in the form of average mileages per vehicle per year.

Table 3 gives these figures for 1952 and 1956. For cars, the mileages in Table 2 were reduced by the mileage of government and military vehicles, and divided by the number of vehicles licensed as cars, as hackneys with not more than 8 seats, or as exempt cars. For motor cycles the calculations were similar; the vehicles included were bicycles (including combinations), £5 class tricycles, and exempt motor cycles. For public service vehicles, the published figures were used for 1952 both for mileages and numbers of vehicles. At the time of writing the figures for 1956 were not available: the mileage in Table 2 was used, and the number of vehicles obtained by altering the 1952 figure in proportion to the change from 1952 to 1956 in hackney vehicles with more than 8 seats and tramcars, obtained from Ministry of Transport figures (1953, 1957). The numbers of goods vehicles given by Glover and Miller (1954) were used for 1952; the corresponding 1956 figure was obtained by scaling up on the basis of figures given by the Ministry of Transport (1953, 1957). The mileages of goods vehicles were those in Table 2 diminished by $0{\cdot}6 \times 10^9$ and $0{\cdot}7 \times 10^9$ miles to eliminate government, military and farmers' vehicles. The mileages for all motor vehicles were obtained from Table 2, reduced to eliminate government, military and farmers' vehicles. The numbers of vehicles were again obtained from the Ministry of Transport (1953, 1957), the only types excluded being government, military and farmers' vehicles (including tractors).

Because of the fuel shortage in 1956, the miles per vehicle in 1956 are slightly lower than similar figures for 1955.

TABLE 3

Estimated Miles per Vehicle per Year

	1952	1956
Motor cycles	3,500+	3,400
Cars and taxis	8,300	7,800
Public service vehicles	30,500−	29,000
Goods vehicles	11,400	11,000
All motor vehicles	8,500−	7,800

Summary and Conclusions

The total mileage travelled by motor vehicles in Great Britain in 1952 is estimated by five largely independent methods, using returns from public service and goods vehicle operators, traffic census records and fuel consumption data. The estimates are broken down by type of vehicle, averaged, and finally scaled up to the 1956 level.

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By comparing these mileages with numbers of licensed vehicles, estimates are obtained of the average miles per vehicle per year for each type.

It is thought that the estimates given, although not very precise, are the best that can be made with the information at present available.

Acknowledgments

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APPENDIX A

Estimation of Composition of Traffic

The object of the following analysis is to derive from traffic-count data the best available estimate of the percentage of the total motor-vehicle-miles in Great Britain in 1952 that was run by each of the four classes of vehicle:

Motor cycles (including motor-assisted pedal cycles and combinations).
Cars and taxis.
Public service vehicles (including trams).
Goods vehicles.

The mileage of pedal cycles as a percentage of that of motor vehicles is also evaluated. The data are drawn from a number of sources, no one of which provides satisfactory estimates of the quantities required. It is therefore necessary to combine the various sets of data.

Some of the data are only available for the year 1954. For convenience, therefore, the analysis is carried out on 1954 data until a late stage in the calculation, when a correction is applied to allow for changes in composition between 1952 and 1954.

Data

(i) Quarterly 3-day counts were carried out in 1954 at each of 10 points on trunk and class I roads. The figures required are given in Table A1.

TABLE A1

Quarterly 3-day Counts on Trunk and Class I Roads

	*a = Total of 10 Points July–Sept. Quarter, Ave. Weekly Flow**	*b = Total of 10 Points Whole Year, Ave. Weekly Flow**	*c, as b but Fridays Only*	*d = b/a*	*e = b/c*
Motor cycles	60,233	46,749	6,333	0·776	7·382
Cars	448,604	382,919	54,688	0·854	7·002
P.s.v's	76,009	70,106	10,429	0·922	6·722
Goods	185,998	182,888	32,404	0·983	5·644
Pedal cycles	50,666	43,575	6,424	0·860	6·783

* 5 × Friday + Saturday + Sunday.

(ii) In August 1954, the Ministry of Transport and Civil Aviation held a 7-day census at about 5,000 points on trunk and class I roads. Results for 300 of these points are given in unpublished tables. The composition of the traffic is given in Table A2.

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TABLE A2

Composition of Traffic, Summer 1954
(300-point Census)

	f = Per cent. of Total Motor Vehicles
Motor cycles	8·47
Cars	58·75
P.s.v's	6·42
Goods	26·36
	100·00
Pedal cycles	11·67

(iii) The sample survey of the distribution of traffic (Tanner, 1957) gave average traffic flows by class of road for weekdays in 1954 at samples of points in three counties and one county borough. The relevant data are given in Table A3.

TABLE A3

Traffic Flows by Class of Road

	g = Per cent. of Total Motor Vehicles, Trunk and Class I Roads Only	*h = Per cent. of Total Vehicles, All Roads*	*j = h/g*
Motor cycles	5·62	6·71	1·194
Cars	54·12	52·48	0·970
P.s.v's	6·61	6·24	0·943
Goods	33·65	34·57	1·027
	100·00	100·00	
Pedal cycles	12·75	28·34	2·224

(iv) Macnaughton-Smith and Tanner (1956) give the best available estimates of trends from 1952 to 1954. From Table I (but using unrounded figures) the figures given in Table A4 are obtained.

TABLE A4

Flow in 1952 as Proportion of Flow in 1954 (k)

Motor cycles	0·874
Cars	0·824
P.s.v's	1·055
Goods	0·928
Pedal cycles	1·218

Analysis of Data

Censuses (i), (ii) and (iii) above may be used as the basis of three largely independent estimates of the composition of traffic in 1954 on the roads as a whole.

In the first place, census (i), adjusted for the fact that only trunk and class I roads are represented, gives an estimate. Multiplying column (*b*) (Table A1) by column (*j*) (Table A3) and reducing to percentages the first column in Table A5 is obtained.

Secondly, an estimate may be obtained from census (ii) if adjustments are made to

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allow for the restriction of the counts to August and to trunk and class I roads. Multiplying (*f*) (Table A2), (*d*) (Table A1) and (*j*) (Table A3) and reducing to percentages the second column in Table A5 is obtained.

Lastly, census (iii) provides an estimate if adjusted to allow for the exclusion of week-ends from the counts. Multiplying column (*h*) (Table A3) by column (*e*) (Table A1) and reducing to percentages the third column in Table A5 is obtained.

TABLE A5

Composition of Traffic, Great Britain, 1954, Estimated by Three Methods

	Percentage of all Motor Vehicles			
	Method (i)	*Method (ii)*	*Method (iii)*	*Weighted Average*
Motor cycles . . .	8·19	8·85	7·57	8·21
Cars and taxis . . .	54·53	54·86	56·18	55·38
P.s.v's	9·71	6·29	6·41	6·83
Goods vehicles . . .	27·57	30·00	29·83	29·58
	100·00	100·00	99·99	100·00
Pedal cycles . . .	14·23	25·16	29·39	25·41

It is considered that the first method is rather less accurate than the other two, and that the best average estimates are obtained by combining the three sets in the ratio 1 : 3 : 3. These averages are given in the last column of Table A5. To reduce the figures to the 1952 level, this column is multiplied by column (*k*) (Table A4). By reducing to percentages a final estimate is obtained (Table A6).

TABLE A6

Estimated Per cent. of All Motor Vehicles, Great Britain, 1952

Motor cycles	8·20
Cars and taxis . . .	52·17
P.s.v's	8·24
Goods vehicles . . .	31·38
	99·99
Pedal cycles	35·38

APPENDIX B

Calculation of Vehicle-miles by Method II
(using figures for total fuel consumption)

Introduction

The following steps are carried out in this appendix:

(1) Estimation of the total quantity of diesel fuel used by goods and public service vehicles in 1952.

(2) Comparison of the figures obtained in (1) with the total deliveries of diesel fuel into consumption. This comparison shows that there is no excessive error in this method.

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(3) Estimation of the total quantity of petrol used by goods and public service vehicles in 1952.

(4) Subtraction of the figure obtained in (3) from the total deliveries of petrol into consumption; the assumption is then made that this petrol is used by motor cycles, cars and taxis.

(5) Calculation of the average petrol consumed per vehicle-mile by solo motor cycles, using data concerning the consumption of the various sizes of engine.

(6) Calculation of the average petrol consumed per vehicle-mile by cars and taxis.

(7) Calculation of the vehicle-miles run by motor cycles and by cars and taxis in 1952 which would consume the petrol calculated in (4), and which would give the same ratio of flows of these two classes of vehicle as has been calculated in Appendix A. These vehicle-miles are the end-product of this method and are combined with the vehicle-miles for goods and public service vehicles.

Consumption of Diesel Fuel

Diesel fuel is almost entirely consumed by goods and public service vehicles. The data in Table B1 are the Laboratory's best estimates of diesel fuel consumption.

TABLE B1

Estimated Consumption of Diesel Fuel by Goods Vehicles and P.s.v's

	m.p.g.
Goods vehicles, pay-load rating:	
2–6 tons	18
6–10 tons	15
12 tons and over	10
P.s.v's Single-deck	14
Double-deck	10

The classes of goods vehicles in this table will be taken as corresponding to unladen weights of: 2–3 tons, 3–5 tons and over 5 tons respectively.

Goods Vehicles

The numbers of diesel goods vehicles in the various classes at September 30th, 1952 may be obtained from Tables 1 and 4 of the paper by Glover and Miller (1954). Information supplied privately gives the average mileages run by diesel-engine vehicles in the important classes of those tables during the week September 22nd–28th, 1952; for the other classes, the mileages in Table 10 of Glover and Miller's paper must be used or values must be estimated. The total vehicle-miles in that week is thus estimated as 30·8 (diesel) and 213·2 (petrol) millions. Combining these data with those for fuel consumed per mile leads to the estimate that in that week goods vehicles consumed 2·320 million gallons of diesel fuel.

In Table 22 of their paper, Glover and Miller give a figure for the total mileage in 1952 which is 44·96 times that calculated above for the one week. It is reasonable therefore to multiply that week's fuel consumption by 44·96 to get the consumption in 1952. This gives 104·3 million gallons of diesel fuel.

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Public Service Vehicles

Tables 1, 8, 11 given by the Ministry of Transport and Civil Aviation (1954) and Table X 23 of the Annual Report of the British Transport Commission (1953) provide most of the data required for p.s.v's. It is convenient to assume (in the absence of definite information) that for operators other than the London Transport Executive, single-deck vehicles do the same mileage per vehicle per year as do double-deck vehicles; and similarly for diesel-, as against petrol-engine vehicles. These assumptions and data, together with Table B1, lead to an estimate of the consumption of diesel fuel by p.s.v's. in 1952 of 163·5 million gallons.

Thus the estimate of the diesel fuel consumed by goods and public service vehicles in 1952 in Great Britain is 267·8 million gallons, and this is 85 per cent. of the total diesel fuel supplied into the United Kingdom for road vehicles in that year (Petroleum Information Bureau, 1954). Many assumptions have been made. It is quite likely that diesel-engine buses and coaches are used on the longer or more arduous routes in preference to petrol-engine vehicles. If this is so it would explain a higher percentage of the total of diesel fuel. Further, data available for London Transport Executive show that double-deck buses average 15 per cent. more miles per vehicle per year than do single-deck buses. If this also applies to the local authority or "other" operators it, again, explains more of the total consumption. There are, of course, other users of diesel fuel; for example, a considerable quantity is used in agricultural tractors and also in Northern Ireland.

As this calculation, based on estimated fuel consumption per vehicle-mile, explains 85 per cent. of the total deliveries into consumption it is considered satisfactory and it gives confidence that a more extensive calculation using petrol will give a figure for petrol-engine vehicle-miles which is not grossly inaccurate.

Consumption of Petrol

An estimate will now be made of the petrol used by goods and public service vehicles and the remainder of the total petrol consumption will be apportioned between cars, taxis and motor cycles so as to estimate their total mileage in 1952. The data in Table B2 are the Laboratory's best estimates of petrol consumption.

Goods Vehicles

The data in Table B2 will be adapted to the unladen weights as follows:

Unladen Weight	*Petrol Consumption (m.p.g.)*
Not over 12 cwt.	35
Over 12 cwt, not over 1 ton	28
„ 1 ton, „ „ 2 tons	18
„ 2 tons, „ „ 3 „	12
„ 3 „ „ „ 5 „	10
Over 5 tons	6

Proceeding as for diesel fuel leads to an estimated consumption of petrol by goods vehicles in the week September 22nd–28th, 1952 of 13,818,000 gallons, and 44·96 times this is 621·3 million gallons.

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TABLE B2

Estimated Consumption of Petrol by Various Classes of Road Vehicle

		m.p.g.
Goods vehicles, pay-load rating:		
Delivery van type	Up to 15 cwt.	28
	15–25 cwt.	18
2–6 tons (unladen wt. below 3 tons)		12
6–10 tons (2 axle, mainly over 3 tons unladen wt.)		10
12 tons and over (multi-axle, over 3 tons unladen wt.)		6
P.s.v's	Single-deck	9
	Double-deck	6
Taxis		20
Cars	Up to 10 h.p.	38
	10–12 h.p.	30
	12–16 h.p.	25
	16–20 h.p.	20
	Over 20 h.p.	15
	Up to 1,100 c.c.	38
	1,101–1,600 c.c.	31
	1,601–2,100 c.c.	25
	2,101–2,700 c.c.	22
	Over 2,700 c.c.	17
Motor cycle combinations		60
Solo motor cycles	Up to 150 c.c.	120
	150–250 c.c.	90
	251–350 c.c.	75
	351–500 c.c.	65
	Over 500 c.c.	60
Motor-assisted pedal cycles		175

Government-owned and farmers' goods vehicles are thought to be mainly petrol-engine vehicles. Their total mileage, estimated as 600 million miles, in 1952 was 6·3 per cent. of all petrol-engine goods-vehicle-mileage, and adding this proportion to the petrol consumed by non-government goods vehicles, the total petrol consumed by goods vehicles in 1952 in Great Britain is estimated as 660·4 million gallons.

Public Service Vehicles

Proceeding as for diesel-engine p.s.v's, the estimate of petrol consumed by p.s.v's in 1952 is 51·37 million gallons. The number of p.s.v.'s in Northern Ireland in 1952 was 1·97 per cent. of that in Great Britain and the number of goods vehicles was 2·31 per cent. (British Road Federation, 1953). The structure of the fleets and the miles per vehicle probably differed considerably from those in Great Britain but it is thought that, as these percentages are small, their use will improve the estimates of petrol consumption in Great Britain; hence $0{\cdot}0197 \times 51{\cdot}37 + 0{\cdot}0231 \times 621{\cdot}3 = 15{\cdot}36$ million gallons of petrol will be considered to be used by goods and public service vehicles in Northern Ireland. Now the total of deliveries of petrol (including motor benzole) into consumption in the United Kingdom in 1952 (Petroleum Information Bureau, 1954) was 1,686·6 million gallons. Hence the total for use in vehicles other than goods and public service was 959·5 million gallons.

Cars and Motor Cycles

Cars and motor cycles will be assumed to be the only other vehicles needing to be considered (cars include taxis).

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Let the numbers of licensed motor-assisted pedal cycles, motor cycles (solo), motor cycles (combinations and tricycles), cars and taxis in Great Britain in 1952 be N_1, N_2, N_3, N_4. Let the average fuel consumptions be b_1, b_2, b_3, b_4 gallons per mile; let the average miles per vehicle per year be a_1, a_2, a_3, a_4; let k be a multiplier. Then $N_1 = 120{,}472$ $N_2 = 674{,}242$; $N_3 = 148{,}505$; $N_4 = 2{,}569{,}209$ (Ministry of Transport, 1953).

From Appendix A and other data,

$$N_1a_1 + N_2a_2 = 0{\cdot}7683 \times 8{\cdot}20\,k \quad . \quad . \quad . \quad . \quad (1)$$

$$N_3a_3 = 0{\cdot}2317 \times 8{\cdot}20\,k \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$N_4a_4 = 52{\cdot}17\,k \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$(N_1a_1b_1 + N_2a_2b_2 + N_3a_3b_3)\,c_1 + N_4a_4b_4c_4 = 959{\cdot}5 \times 10^6 \quad . \quad . \quad (4)$$

where c_1 is the ratio of motor cycle flow in the United Kingdom to that in Great Britain, and c_4 is the ratio for cars and taxis. As we have no data on traffic flows for Northern Ireland these ratios will be taken as the ratios of the numbers of vehicles (British Road Federation, 1953), and as the proportions in Northern Ireland are small, this will be a fair approximation. Thus c_1 and c_4 will be taken as 1·0141 and 1·0226 respectively.

From the ratios of motor cycle flows to numbers in 1953 (the flow data not being available for 1952),

$$a_2 = 1{\cdot}524\,a_1 \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

The values of b_1 and b_3 are given by Table B2; b_2 and b_4 must be calculated as weighted averages.

Average petrol consumption for solo motor cycles (b_2).—The larger motor cycles are thought to do more miles per year than the smaller ones. As the extent of this difference is not known a term depending on a parameter r will be included so that the effect of this difference can be seen. The calculation is set out in Table B3.

TABLE B3

Average Petrol Consumption per Mile for Solo Motor Cycles (Over 60 c.c.)

Size of Motor Cycle (c.c.)	*Number (Thousands)*	*Consumption Gal./Mile*	*Weighting Term*	*Product*
60–150	206	1/120	$1 - r$	$1{\cdot}717\,(1 - r)$
151–250	139	1/90	1	1·544
Over 250	326	1/65	$1 + r$	$5{\cdot}015\,(1 + r)$
	671*			$8{\cdot}276 + 3{\cdot}298\,r$

* 671,000 differs from N_2 above because the latter includes exempt and Crown motor cycles, the sizes of which are not given.

The mean petrol consumption per motor-cycle-mile is

$$\frac{8{\cdot}276 + 3{\cdot}298\,r}{671 + 120\,r} \text{ gallons.}$$

For various values of r this is

$r =$	0	0·1	0·2	0·3
Mean petrol consumption =	0·0123	0·0126	0·0129	0·0131

The value of 0·013 gallon per mile will be taken for b_2.

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Average petrol consumption for cars and taxis (b_4).—A weighting term will also be introduced for cars and taxis to make some allowance for differing average mileage per vehicle per year between cars with differing engine sizes. From Table B2 and Part IIb of the Ministry of Transport paper (1953), Table B4 is obtained.

TABLE B4

Average Petrol Consumption per Mile for Cars and Taxis

Size of Engine	*Number (Thousands)*	*Consumption (Miles/Gallon)*	*Weighting Term*	*Product*
0–9 h.p.	684	38	1	18·00
10–12	776	30	$1 + r$	$25{\cdot}87(1 + r)$
13–16	202	25	$1 + 2r$	$8{\cdot}08(1 + 2r)$
17–20	52	20	$1 + 3r$	$2{\cdot}60(1 + 3r)$
21–	53	15	$1 + 4r$	$3{\cdot}53(1 + 4r)$
0–1,100 c.c.	128	38	1	3·37
1,101–1,600	330	31	$1 + r$	$10{\cdot}65(1 + r)$
1,601–2,100	104	25	$1 + r$	$4{\cdot}16(1 + r)$
2,101–2,700	121	22	$1 + 2r$	$5{\cdot}50(1 + 2r)$
2,701–	58	17	$1 + 4r$	$3{\cdot}41(1 + 4r)$
Taxis	53	20	$1 + 10r$	$2{\cdot}65(1 + 10r)$
	2,561			$87{\cdot}82 + 129{\cdot}90r$

The mean petrol consumption per mile for cars and taxis is

$$\frac{87{\cdot}82 + 129{\cdot}90\,r}{2{,}561 + 2{,}986\,r} \text{ gallons.}$$

For various values of r this is

$r =$	0	0·1	0·2	0·3	0·4	0·5
Mean petrol consumption =	0·0343	0·0352	0·0360	0·0367	0·0372	0·0377

As no reliable evidence is available on which to choose the best value of r it is fortunate that, to the overall accuracy expected, the value of the mean petrol consumption does not vary rapidly with r. A mean consumption of 0·036 gallon per mile will be taken as the value of b_4.

Conclusions

Using these figures in equations (1) to (5) gives

$$k = 472{\cdot}5 \times 10^6 \text{ miles, and}$$
$$a_1 = 2{,}590 \text{ miles}$$
$$a_2 = 3{,}950$$
$$a_3 = 6{,}040$$
$$a_4 = 9{,}590.$$

Hence the total flows in Great Britain in 1952 are estimated as

Motor-assisted pedal cycles	$0{\cdot}3 \times 10^9$ miles
Solo motor cycles	2·7
Combinations and tricycles	0·9
Total for motor cycles	3·9
Cars and taxis	24·6
P.s.v's (diesel, petrol, electric)	2·5
Goods vehicles	11·6
Total vehicle-miles	$42{\cdot}6 \times 10^9$

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APPENDIX C

Overall Fuel Consumption Rates

The gallons consumed per vehicle-mile for motor cycles, cars and taxis, public service vehicles and goods vehicles are required for method III and will now be derived from Appendix B.

Motor Cycles

In the notation given in Appendix B (p. 434) the average petrol consumption per vehicle-mile for all classes of motor cycle (including motor-assisted pedal cycles) is

$$\text{Average petrol per mile} = \frac{N_1a_1b_1 + N_2a_2b_2 + N_3a_3b_3}{N_1a_1 + N_2a_2 + N_3a_3} \text{ gallons}$$

$$= \frac{0{\cdot}10875\,k}{8{\cdot}20k} = 0{\cdot}01326 \text{ gallon.}$$

Cars and Taxis

The average figure for petrol per mile for cars and taxis was evaluated in Appendix B as 0·036 gallon.

Public Service Vehicles

In the form in which it is required for method III the average fuel per vehicle-mile is equal to the total diesel fuel and petrol consumed, divided by the total public service vehicle-miles (including those of trolleybuses and trams).

From Appendix B,

Estimate of diesel fuel used by public service vehicles in 1952 = 163·5 million gallons.

Estimate of petrol used by public service vehicles in 1952 = 51·37 million gallons.

Total public service vehicle-miles in 1952 (Ministry of Transport and Civil Aviation, 1954)

$$= 2{,}530 \text{ million.}$$

Hence average fuel per public service vehicle-mile

$$= \frac{214{\cdot}87}{2{,}530} = 0{\cdot}08493 \text{ gallon.}$$

Goods Vehicles

Estimate of diesel fuel used by goods vehicles from September 22nd–28th, 1952 = 2·320 million gallons.

Estimate of petrol used by goods vehicles from September 22nd–28th, 1952 = 13·818 million gallons.

Estimate of total goods vehicle-miles from September 22nd–28th, 1952 = 30·8 + 213·2 millions.

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Hence average fuel per goods vehicle-mile

$$= \frac{16 \cdot 138}{244 \cdot 0} = 0 \cdot 06615 \text{ gallon.}$$

Appendix D

Estimates of Total Vehicle-miles, and Their Sources

	Method				
	I	*II*	*III*	*IV*	*V*
Total vehicle-miles in Great Britain in 1952 = $10^9 \times$	35·6	42·6	41·2	42·9	40·1
Sources					
Public service and goods vehicle statistics: (Ministry of Transport and Civil Aviation, 1954), (Glover and Miller, 1954)					
(*a*) Total vehicle-miles	I	..	..	..	..
(*b*) Vehicle-miles in classes	..	I	..	..	..
Relative traffic flows:					
(*a*) Classified and unclassified roads	..	..	..	..	m
(*b*) Weekdays and week-ends	..	..	..	m	..
Composition of traffic:					
(*a*) All roads, cars and motor cycles only	..	m	..	..	..
(*b*) All roads, all vehicles	I	..	I	..	..
Statistics of fuel consumption and supplies: (Petroleum Information Bureau, 1954)	..	I	I	..	..
Empirical relation between vehicle-miles and population derived from 4 areas (Tanner, 1957)	..	..	..	I	..
Flow at 50 points selected as a stratified random sample (Tanner, 1957)	..	..	..	..	I

Notes.—For each source, (*b*) is generally less reliable than (*a*).
I—denotes that the source is used in the method to an important degree.
m—denotes that the source is used in the method to a minor extent.

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A Survey of Fellows—I

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1. Introduction

At the end of March, 1957 questionnaires headed "Enquiry on the Utilisation of Statistical Methods" were sent to all Fellows of the Royal Statistical Society and Fellows and Associates of the Association of Incorporated Statisticians resident in Great Britain. The main purpose of this postal survey was to investigate the extent to which statistical techniques are applied in various fields and the amount of research into their theory and application which is being pursued. In this paper the results of an analysis of answers to Section I of the questionnaire are presented while in a further paper answers to Section II will be discussed. The questionnaire is given in an appendix. Section I was completed by all respondents and information given concerned present and past employment, salary and qualifications.

1.1. *Background and Object*

In 1834 the Royal Statistical Society was founded, having as its object to process, arrange, and publish "facts calculated to illustrate the condition and prospects of society". When the *Journal* was established in May, 1838, as the systematic study of statistics was of very recent origin it was thought necessary to explain what this study involved. It was said to differ from political economy which had the same end in view, "in that it neither discusses causes, nor reasons upon probable effects but seeks only to collect arrange and compare the class of facts which can alone form the basis of correct conclusions with respect to social and political government". In his address at the Jubilee meeting of the Society in 1885 Dr. Mouat spoke of "the watchful jealousy with which the Founders of the Society endeavoured to prohibit the inclusion of opinions among the facts to the collection of which they thought the Society bound to confine itself".

Since these early days a complete revolution has taken place in what is considered to be the basic nature of the science of statistics. Its applications are manifold, there being few fields in which the statistical method has not proved its worth. And, perhaps of even greater importance, there has been built up the mathematical theory of statistics which

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has completely changed the role of the statistician. The silent compiler of tables can now predict, delve for causes, and participate in decision-taking.

This revolution had its beginning in the development of probability theory in the seventeenth century but only in 1869 did the Society accept mathematical statistical methods as a legitimate part of its coverage. Before 1885 only fifteen theoretical papers had been published in the *Journal*. As the theory was developed and applied in more and more fields, so the Society expanded from its initial membership of 300 to a membership of 2,800 at the end of April, 1957. The Manchester Statistical Society which came into existence in 1830 was the first of several municipal societies, and a second national statistical organization was founded in 1948 when the Association of Incorporated Statisticians was incorporated under the Companies Act. This organization—a professional body rather than a learned society—came into existence in an endeavour to help in the training of more persons in "the principles of statistics and their application to the problems of administration and research which arise in industry and commerce, government departments and in all fields of applied science". It was felt that the shortage of statisticians was so acute that some new machinery for training must immediately be provided. At that time it was said in the Report of the Committee on the Provision for Social and Economic Research (Cmd. 6868) that "a chronic struggle is going on between the universities, business and the public service for the services of a supply of statisticians which is in the aggregate equal to only a tiny fraction of the demands immediately manifesting themselves, and still less of the demands which are certain to accrue".

In the past ten years the supply of statisticians has increased but has still by no means caught up with the demand. This survey was undertaken in an attempt to gain a comprehensive picture of the present state of the profession: to enquire into the nature of the work of the statistician, his place of employment, salary and qualifications. Apart from the general interest of such data to statisticians and potential statisticians it was hoped that they might prove of practical value both to universities and to the statistical societies in providing information of relevance to the planning of training programmes.

1.2. *Coverage*

In planning a survey of statisticians there were two immediate problems: how to define a statistician and how to contact him. Furthermore, I had to decide whether to conduct a sample survey or a complete census and whether to use the postal questionnaire or the personal interview. Assuming I wished to have results concerning or representative of all those persons applying statistical techniques in Great Britain, then because of the size of this population of statisticians and the variation in the nature of the work, the personal interview method would be very costly.

Having therefore for reasons of finance decided on the use of a postal questionnaire I next had to decide to whom this questionnaire should be sent. Various approaches were possible according to whether I worked through statistical societies, firms, universities, or a combination of all three. An approach through the firm, which would have involved contacting a sample of firms, was decided against as being too expensive. Working through lists of university qualified statisticians, on the other hand, would eliminate an essential part of the population—those without university qualifications in statistics—and also provide me with many out-of-date addresses.

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The third possibility, that of sending the questionnaires to Fellows of the statistical societies, seemed the most appropriate method as lists of names and addresses were available. The Manchester Statistical Society was not, however, included as it contained very few statisticians not belonging to one of the other societies. It would have been possible to have enlarged the population of the enquiry by asking Fellows for names of statisticians known not to belong to the statistical societies, and repeating this procedure several times I could most certainly have increased the number of statisticians covered by the survey. There would still not have been complete coverage, however, and I felt that to have complete coverage of the enumerable set of Fellows would be more satisfactory.

It was found that there were 2,090 Royal Statistical Society Fellows with addresses in Great Britain and of Fellows and Associates of the Association of Incorporated Statisticians there were 107 who were not Fellows of the Royal Statistical Society. A full census of this total of 2,197 persons was decided upon.

The group covered by this enquiry does not therefore include all persons characterized as "statisticians" in occupational classifications, especially of the clerical grades. On the other hand, it certainly covers almost completely all those individuals who have a formative influence on teaching, research, and the development of applications in new fields. It includes, in addition, a considerable number of those who might be termed practitioners and consumers of statistics.

1.3. *Response and Non-respondents*

From a total of 2,197 questionnaires the number returned completed and available for analysis was 1,211 or 55 per cent. However, the total number of replies received was 1,586, or 72 per cent., the additional 375 replies consisting mainly of letters from people no longer engaged in, or having never been employed on, statistical work. A large proportion of these were from retired persons, and in cases where the respondent was not involved in statistical work of any kind and did not wish to complete any section of the questionnaire, no attempt was made at persuasion, although it had been hoped that Section I would be completed by all who were in employment.

In some cases, although a questionnaire had been sent out to an address in the United Kingdom it was returned from another country by a respondent who was found to have been working abroad for some time. Where the time abroad was greater than six months or when information given was related only to this work, the questionnaire was not used in the analysis. This occurred in only a small number of cases.

Questionnaires were sent out at the end of March, 1957 and 500, or nearly 25 per cent., completed forms were returned in the following threee days. At the end of 10 days a total of 762 had been received and by the end of the month the inflow was at the rate of only two or three a day. On May 5th, when 1,158 replies (of which 1,022 were completed questionnaires) had been received, a letter of reminder was sent out to the 1,039 respondents from whom no reply had been received. In response to this reminder a total of 428 replies, of which 189 were completed questionnaires, were received by June 3rd. These figures expressed as percentages of the total number of questionnaires sent out, are shown in Table 1.

The reminder brought in a smaller proportion of completed questionnaires than had been obtained initially, over 50 per cent. of the second stage replies being letters from

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retired persons and those not employed on work of a statistical nature. This suggests that the residual non-response of 28 per cent. may have been primarily among this class of persons. Letters objecting to the survey or to particular parts of the questionnaire were received from only four or five persons.

TABLE 1

Response to the Questionnaire

	Letters		*Completed Questionnaires*		*Total Replies*	
Stage of Response	*No.*	*%*	*No.*	*%*	*No.*	*%*
Before reminder . . .	136	6·2	1,022	46·5	1,158	52·7
After reminder	239	10·9	189	8·6	428	19·5
Total	375	17·1	1,211	55·1	1,586	72·2

Of the 611 persons who neither completed a questionnaire nor answered by letter, 34 belonged to the Association of Incorporated Statisticians and 577 to the Royal Statistical Society. The year of election to the Society of these 577 non-respondents is shown in Table 2, although further details of age or profession are not available.

TABLE 2

Years of Election of Non-Respondents

1900–09	1910–19	1920–29	1930–39	1940–49	1950–57	*Total*
4	12	31	70	220	240	577

1.4. *The Questionnaire*

In deciding to include in my survey all Fellows of the Royal Statistical Society and of the Association of Incorporated Statisticians, I realized that I was sending questionnaires to many persons not necessarily engaged in statistical work, and therefore not belonging to the class in which I was primarily interested. As, however, the questionnaire was to include questions on age, salary, qualifications and employment, the answers to which would provide interesting information on membership of the societies, these questions were grouped together to form the first section of the questionnaire to which a response from all Fellows was hoped for.

In Section II of the questionnaire questions were asked concerning the application of statistical techniques, research of a statistical nature, readership of statistical books and journals and attendance at meetings of the Royal Statistical Society or the Association of Incorporated Statisticians. Answers to Section II were requested from all those who in Section I had stated that they did require some knowledge of statistical techniques. Those who answered this section were not necessarily employed as statisticians and it is possible that some statisticians employed only in obtaining and tabulating data did not complete the section.

In Section III were questions to be answered only by those teaching or doing research in schools, colleges or universities.

The questionnaire was piloted by sending it out to a chosen sample of 25 Fellows who were asked both to complete the questionnaire and supply suggestions for its improvement. In choosing the statisticians to be included in this small pilot survey, the merits

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of a more representative sample of Fellows were sacrificed for the benefits of the helpful comments which were in fact obtained. I would like to take this opportunity of thanking those who co-operated with me in this stage of the survey, and in particular to my colleague Mr. C. A. Moser and the Director of the Research Techniques Unit, Professor M. G. Kendall, without whose support the enquiry could not have been carried out. I am also very grateful for the clerical help given me by the statistical societies in providing sets of addressed envelopes.

2. Age, Sex and Employment

2.1. *Age and Sex*

Of the 1,211 persons completing Section I of the questionnaire, there were 71 women and 1,140 men. The age distribution for men, women and all persons, is given in Table 3, which also shows the percentage of all persons in each age category.

TABLE 3

Age and Sex of Respondents

		Age in Years at Next Birthday							
Sex	*No Reply*	20–24	25–29	30–39	40–49	50–59	60–69	*Over 69*	*Total*
Men . . .	1	14	140	438	336	157	49	5	1,140
Women . . .	—	5	12	22	25	6	1	—	71
Total No. . .	1	19	152	460	361	163	50	5	1,211
Percentage . .	0·1	1·6	12·6	38·0	29·8	13·5	4·1	0·4	100·1

The modal age group of the men was 30–39 years and of the women 40–49 years but the median age of the men was 38·9 years compared with 38·0 years for the women. Over 50 per cent. of all respondents were under 40.

Eighty-one per cent. of all respondents had been born in England, 6·2 per cent. in Scotland, 3·4 per cent. in Wales, 1·2 per cent. in Ireland, 4·5 per cent. elsewhere in Europe and 3·5 per cent. outside Europe. Three persons did not reply to the question asking for place of birth.

2.2. *Industry or Profession*

The information provided in answer to Question 3 of the questionnaire related to present paid employment and was classified in two ways. First of all a classification according to industry or profession was carried out, and then a second classification according to the occupation or nature of the work of the respondent. In Table 4 the 22 employment groups used are shown, together with the number and percentage of all respondents in each. The number of women in each group is also shown. The scientific Civil Service has been separated from other branches of the Civil Service, although different branches of the non-scientific Civil Service have not been distinguished. The miscellaneous category included employment in the regular army, work as a general practitioner, writing novels, and the law.

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TABLE 4

Profession or Industry in which Employed

No. of Group	*Industry or Profession*	*No. of Women*	*Total No. Persons*	*%*
1	Civil Service—scientific	3	53	4·4
2	Civil Service—non-scientific	13	132	10·9
3	Local Government Service	6	34	2·8
4	Public Utilities	1	73	6·0
5	Public Corporations	4	47	3·9
6	Social and Economic Research or Administration	2	58	4·8
7	Psychological and Medical Research or Administration	4	38	3·1
8	Other Scientific Research	3	41	3·4
9	Universities	16	163	13·5
10	Non-university Teaching	1	24	2·0
11	Advertising and Market Research	2	42	3·5
12	Banking, Insurance, Accountancy, Computing	0	134	11·1
13	Wholesale and Retail Distributive Trades	3	22	1·8
	Manufacturing Industry—			
14	Electrical and electronic goods	3	38	3·1
15	Engineering tools, machines	1	38	3·1
16	Alcohol, chemicals, rubber, plastics etc.	4	94	7·8
17	Soap, detergents, food, tobacco	1	43	3·6
18	Paper, cotton, and woollen goods	2	34	2·8
19	Iron, steel and other metals	0	37	3·1
20	Oil Extracting, Refining, Distribution	1	29	2·4
21	Miscellaneous	1	23	1·9
22	None	0	14	1·2
	Total	71	1,211	100·2

From Table 4 it may be seen that the three largest employment groups, which each contain over 10 per cent. of all respondents, are Universities, Banking etc., and the non-scientific Civil Serivce. The largest of the different manufacturing groups relates to alcohol, chemicals, rubber and plastics which with 94 persons, or 7·8 per cent., is the fourth largest single employment group.

The 71 women respondents were heavily over represented in the non-scientific Civil Service, and Universities, and in nearly all other categories were under-represented, there being none in the large Banking group.

Seven broader employment categories were obtained from the 22 listed in Table 4 and these were used in some cross-classifications where the numbers in cells would otherwise have become too small. These seven categories, together with the identification numbers of the groups of Table 4 which they include, are shown in Table 5.

TABLE 5

Main Employment Categories

No.	*Employment Category*	*Groups of Table 4 Included*	*No of. Women*	*Total No. Persons*	*%*
I	Civil Service	1, 2	16	185	15·3
II	Local Govt. Public Utilities and Corpns.	3, 4, 5	11	154	12·7
III	Research Bodies	6, 7, 8	9	137	11·3
IV	Universities	9	16	163	13·5
V	Finance and Commerce	11, 12, 13	5	198	16·4
VI	Manufacturing Industry	14–20	12	313	25·9
VII	Miscellaneous and None	10, 21, 22	2	61	5·0
	Total	—	71	1,211	100·1

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Of the seven main employment categories the largest is Manufacturing Industry, containing 25·9 per cent. of all respondents and 12 women; that is, 16·9 per cent. of all women.

The age distribution within each of the seven employment categories is shown in Table 6. Manufacturing Industry with 19·8 per cent. of its members under 30 and only 9·6 per cent., 50 and over, contains the youngest group of respondents, while (ignoring the Miscellaneous category, which is too mixed to be of interest) Finance and Commerce contains the oldest, with 8·1 per cent. under 30 and 27·7 per cent. over 50.

TABLE 6

Age and Employment Category

Age in Years	*I* (%)	*II* (%)	*III* (%)	*IV* (%)	*V* (%)	*VI* (%)	*VII* (%)	*All Groups* (%)
22–24	1·1	0·6	1·5	2·5	1·0	1·6	4·9	1·6
25–29	8·6	11·7	15·3	13·5	7·1	18·2	6·6	12·6
30–39	39·5	37·7	34·3	37·4	32·3	46·3	19·7	38·0
40–49	37·3	26·0	28·5	31·9	31·8	24·3	36·1	29·8
50–59	13·0	21·4	13·1	9·8	19·6	6·4	21·3	13·5
Over 59	0·5	2·6	7·3	4·9	8·1	3·2	11·5	4·6
Total %	100	100	100	100	99·9	100	100·1	100·1
Total No.	185	154	137	163	198	313	61	1,211
Median age	42·4	39·6	39·4	38·7	42·6	36·1	44·0	39·0

Of the 22 employment groups, Group 12 (Banking, Accountancy, etc.) with 67·9 per cent. of all members over 40, contains the largest proportion of respondents in this category. Local Government Service (3) with 67·6 per cent. comes next, followed by Social and Economic Research (6) with 65·5 per cent.

2.3. *Nature of Work*

The basic nature of the skills required by respondents in their present jobs was inferred from their answers to Questions 3, 6, and 17. This occupational classification was felt to give more relevant information on the work being done by respondents than would be provided merely by the distributions by industry and qualifications, although it is obviously of a less factual nature.

The Operational Research category was used only for those describing their work to be of this nature, and the Administrative category only for those whose work appeared to require no specialized knowledge of statistics, economics, or the other techniques separately listed.

TABLE 7

Nature of Work

Nature of Work	*No. Women*	*Total No.*	%
Statistical	48	452	37·3
Scientific or technical	10	157	13·0
Economic	6	107	8·8
Teaching (not statistics, science or economics)	3	45	3·7
Operational Research and Programming	2	60	5·0
Actuarial, financial	0	143	11·8
Administrative, managerial	1	229	18·8
Miscellaneous	1	18	1·5
Total	71	1,211	99·9

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Not surprisingly, the largest category of respondents, forming 37·3 per cent. of the total number, were found to be doing statistical work and 67·6 per cent. of the women were in this category. The next largest category was that of those doing administrative work, comprising 18·8 per cent. of all respondents. The age distribution and median age within each of these categories is shown in Table 8. The median age of the Operational Research group, at 33·9 years, is the lowest, with the statisticians next at 36·4. The actuarial group had the highest median age of 46·2, apart from the miscellaneous category.

TABLE 8

Age and Type of Work

Age in Years	*Statistical* (%)	*Scientific* (%)	*Economic* (%)	*Administrative* (%)	*Actuarial* (%)	*Op. Research* (%)	*Teaching* (%)	*Miscellaneous* (%)	*Total All Groups* (%)
20–24 . .	2·9	1·3	1·9	—	0·7	1·7	—	—	1·6
25–29 . .	16·6	15·9	13·1	3·9	5·6	25·00	13·3	—	12·6
30–39 . .	44·2	41·4	43·9	26·2	26·6	55·0	28·9	22·2	38·0
40–49 . .	24·6	28·0	30·8	38·9	34·3	15·0	44·4	33·3	29·8
50–59 . .	9·1	10·2	7·5	22·7	25·9	1·7	6·7	27·8	13·5
60 and over .	2·6	2·5	2·8	8·3	5·6	1·7	6·7	16·7	4·5
No reply .	—	0·6	—	—	1·4	—	—	—	0·1
Total % .	100·0	99·9	100·0	100·0	100·1	100·1	100·0	100·0	100·1
Total No. .	452	157	107	229	143	60	45	18	1,211
Median age	36·4	37·4	37·6	44·7	46·2	33·9	41·5	58·7	39·0

The median age for women doing statistical work was 37·5, slightly higher than that of the men.

In reply to Question 6 of the questionnaire 46·5 per cent. of respondents stated that their duties were *principally* concerned with organization and management. The 334 persons in this category who were not placed in the Administrative category in Table 8 were in their managerial capacity requiring either statistical, economic, actuarial or scientific knowledge. The age distribution for answers to Question 6 is given in Table 9. The median age of those performing managerial work is still considerably higher than that of the non-managerial class, although lower than that of the administrative class in Table 8.

TABLE 9

Managerial Work and Age

	Age									
Type of Work	20–24 (%)	25–29 (%)	30–39 (%)	40–49 (%)	50–59 (%)	60 *and Over* (%)	*No Reply* (%)	*Total* (%)	*Total No.*	*Median Age*
Managerial . .	0·5	6·9	32·9	35·2	17·8	6·7	—	100·0	563	42·3
Non-managerial .	2·5	17·8	43·0	25·2	9·1	2·3	0·2	100·1	628	36·4

Of the women respondents only 13, or 18·3 per cent., were doing work of a managerial nature, 8 being aged between 30 and 40, 4 between 40 and 50 and one between 50 and 60 years.

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2.4. *Movement Across Industrial and Occupational Classes*

In Questions 4 and 5 of the questionnaire, information relating to previous jobs was asked for, in order to obtain some idea of the movement in and out of statistical work. Of the 452 persons doing statistical work, 277 had also been doing statistical work in their last job. Of these 277 persons, 222 had at some time had a job which was non-statistical. Also, 126 persons now doing statistical work had in their last job been doing non-statistical work. Thus of the total persons doing statistical work 10·8 per cent. were in their first job and 77·0 per cent. had at some time been engaged on non-statistical work. The possible inflation of this figure by directed work during war-time should not be overlooked, although ordinary service in the Forces was not included.

Looking at the other side of the picture, of the 759 respondents not primarily employed on statistical work in their present job, 115 had in their last job been so employed, and a further 70 persons had at some time been engaged in statistical work. Thus of the total 1,211 respondents, 574 or 47·4 per cent. had never been employed primarily on statistical work and 15·3 per cent. had at one time been employed on statistical work but were in their present job not so employed.

The movement across the 22 industrial or professional classes was also considered. Of the 76 persons whose last job had been in a university 36 moved to a non-university appointment and 40 to an appointment in a different university.

Change of status within a university was not taken into account in this classification as it had obviously not been registered by all respondents. Fifty-five persons had moved from a non-university to a university appointment. Of the total moves 52·5 per cent. were moves across the industrial categories and 47·5 per cent. were within categories.

3. QUALIFICATIONS

3.1. *University Attended*

In Question 8 and Question 9 of the questionnaire respondents were asked for information concerning their qualifications. The replies to these questions are presented in this section. Information about the first B.Sc. or B.A. degree is shown in Tables 10 and 11. Table 10 shows the university attended and Table 11 the main subject of the degree.

TABLE 10

University of First Degree

	University								
	None	*Cambridge*	*Oxford*	*London*	*Other English*	*Scottish*	*Irish Welsh*	*Other*	*Total*
Men . . .	261	190	88	351	140	50	29	31	1,140
Women . .	8	9	9	33	5	4	1	2	71
All . . .	269	199	97	384	145	54	30	33	1,211
Percentage all respondents .	22·2	16·4	8·0	31·7	12·0	4·5	2·5	2·7	100·0

22·2 per cent. of all respondents and 11·2 per cent. of women respondents were without any degree. Nearly half of the women were graduates of London University as compared with 31·7 per cent. of all respondents. The group next largest in size was that of Cambridge graduates, comprising 16·4 per cent. of all respondents.

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3.2. *Special Subject*

From Table 11 it is seen that mathematics was most frequently the main subject of the first degree, with economics next and statistics, in which only 9·3 per cent. of all respondents had specialized, a poor third.

The proportion of Fellows qualifying in statistics shows a gradual increase since 1930–39 whereas the proportion qualifying in economics has declined. The proportion of mathematics graduates shows a sharp increase followed by a slight decline.

The figures reflect quite clearly the original interest of economists in statistics, the development of statistical mathematics, and the gradual but slow increase in the provision of degree courses with statistics as a special subject. At the present moment only at University College, London may a degree in statistics be obtained. For the purpose of this survey, however, degrees in mathematics or economics in which statistics had been taken as the principal special subject (as is possible at the London School of Economics and at Imperial College) were classified as qualifications in statistics.

In the case of many other Fellows with qualifications in economics, mathematics, natural science, or technology, optional or compulsory elementary courses in statistics may have been included in their degree course.

TABLE 11

Subject and Year of Graduation

	Year of Graduation							
Special Subject	*Before 1920*	*20–29*	*30–39*	*40–49*	*50–56*	*No Date*	*Total Number*	*Percentage of all Respondents*
Mathematics	2	22	102	128	88	2	344	28·4
Economics	1	27	64	73	54	2	221	18·3
Statistics	0	2	17	38	56	0	113	9·3
Technology	2	8	6	9	1	1	27	2·2
Other Science	4	20	33	38	12	2	109	9·0
Arts	3	6	15	14	9	0	47	3·9
General Degree	4	7	21	25	20	2	79	6·5
No degree	—	—	—	—	—	271	271	22·4
All Subjects	16	92	258	325	240	280	1,211	100·0

TABLE 12

Special Subject in Second Degree

	Second Degree							
First Degree	*Maths.*	*Stats.*	*Econ.*	*Sc. & Med.*	*Arts*	*Technol.*	*General*	*Total*
Mathematics	19	9	12	4	7	—	—	51
Statistics	—	1	2	—	—	—	1	4
Economics	—	8	8	2	2	—	—	20
Science & Medicine	4	5	1	12	1	1	—	24
Arts	—	—	6	—	5	—	—	11
Technology	2	1	1	—	—	—	—	4
General	8	2	2	2	—	1	1	16
Total	33	26	32	20	15	2	2	130

As can be seen in Table 12, 130 persons had taken a further Bachelor's degree, 33 specializing in mathematics, 32 in economics and 26 in statistics.

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In respect of other degrees and diplomas, 167 persons had obtained a Master's degree and 124 a Ph.D. Statistical diplomas and certificates were held by 128 persons, 14 of whom held the Oxford Diploma, 27 the Cambridge Diploma, 53 qualifications of the Royal Statistical Society and 34 qualifications of the Association of Incorporated Statisticians. A total of about 30 per cent. of all respondents had some qualification in statistics.

Of the Operational Research workers 50 per cent. had degrees in mathematics and 15 per cent. in statistics.

3.3. *Class of Degree*

In Table 13 the class of the first degree held is shown. Non-response to this question was quite high as 57 persons who had a degree did not state the class. 35·5 per cent. of all respondents held second class degrees, 21·8 per cent. first, and 15·9 per cent. thirds or pass degrees. The percentage of women with firsts was only 12·7, while the percentage with seconds was 49·3.

TABLE 13

Class of First Degree

	Class					
	First	*Second*	*Third*	*Pass*	*None or No Reply*	*Total*
Men . .	255	395	57	118	316	1,050
Women . .	9	35	8	9	10	71
All . . .	264	430	65	127	326	1,121
Percentage of all respondents .	21·8	35·5	5·4	10·5	26·9	100·1

Of those graduates stating the class of their degree, the percentage with firsts was 29·8, with seconds 48·5 and with thirds or pass degrees 21·7. These figures, for respondents, as is to be expected, compare favourably with figures for all those graduating in Great Britain in a particular year. Thus in the sample survey of all men who graduated in 1950, carried out by Political and Economic Planning* it was found that of the 3,961 graduates answering the questionnaire 12 per cent. had obtained first class degrees, 47 per cent. second class degrees, and 41 per cent. third class or pass degrees.

Of respondents, 154 men and 4 women had obtained their degrees as part-time students.

4. SALARY

4.1. *Salary by Age*

In Question 7(a) respondents had indicated their gross salary to be in one of six ranges. It seems probable that non-response will have introduced some bias into results obtained from this question. The most highly paid Fellows may have been those who failed to reply to the questionnaire. It seems equally likely, however, that many of the least well-paid also failed to reply and that the average has a smaller error than the dispersion.

* *Graduate Employment, Political and Economic Planning*, p. 39. Allen and Unwin Ltd., 1956.

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TABLE 14

Percentage Numbers of Salaries in Given Ranges

Salary (£)	*Age* 20–24 (%)	25–29 (%)	30–39 (%)	40–49 (%)	50–59 (%)	60–69 (%)	*Over* 69 (%)	*No Reply* (%)	*All Ages* (%)
Under 750	89·5	23·7	4·1	1·4	4·3	6·0	—	—	7·2
750–	10·5	42·8	10·9	4·4	1·8	6·0	—	—	11·5
950–	—	20·4	22·8	8·0	3·1	6·0	—	—	14·3
1,150–	—	7·2	28·5	15·2	12·9	6·0	—	—	18·3
1,450–	—	3·9	24·6	31·6	23·3	14·0	20·0	—	23·0
2,000–	—	0	8·0	37·9	49·1	60·0	60·0	100	23·8
No reply	—	2·0	1·1	1·4	5·5	2·0	20·0	—	2·0
Total %	100·0	100·0	100·0	99·9	100·0	100·0	100·0	100·0	100·1
Total No.	19	152	460	361	163	50	5	1	1,211
Median salaries	—	£870	£1,274	£1,805	Over £2,000	Over £2,000	—	—	£1,430
Median salaries of women	—	£750	£1,025	£1,250	£1,725	—	—	—	£1,223

In comparing salaries of different groups of respondents medians were used, as being more suitable than means, both for ease of calculation and interpretation. The percentage of persons in some groups falling in the open end intervals also suggested the median as a better measure. From Table 10 we see that the two largest salary groups were £1,450 to £2,000, and £2,000 and over, containing respectively 23·0 per cent. and 23·8 per cent. of all respondents. The large number of persons with salaries falling in the highest of the salary ranges suggests that an additional division would have been advisable. The median salary within the age groups increases steadily with a large jump from £1,274 to £1,805 as we pass from age range 30–39 to 40–49.

Median salaries of women are shown separately in Table 14 and can be seen to be considerably lower than that of all respondents, within each age group, the difference being at a maximum in the age range 40–49, in which range 137 men and no women had over £2,000. As 25 of the women are employed in the Civil Service, in which equal pay for women and men will have been realized by 1961, this difference should become smaller in the near future.

4.2. *Salary by Industry*

A cross classification of salary according to industry or profession was made and results are shown in Table 15 using the 22 industrial groups. The same data are used in Table 16 which shows, for each of the seven main employment categories, the percentage of persons in each of the salary ranges. Median salaries and median ages within each group and category are also shown.

Of the 22 industrial groups, that with the highest median salary of over £2,000 is group 12 (Banking, Insurance etc.), but this group has a fairly high median age of 44·9 years. Similarly group 6 (Social and Economic Research) and group 13 (Wholesale and Retail Distributive Trades) have high median salaries, of £1,852 and £1,794, but also high median ages of 44·8 years and 46·9 years. Industrial groups in which median salaries are high and median ages average or low are groups 17 and 11. Group 17 (manufacture of soaps,

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detergents, food and tobacco) contains 43 persons of median age 38·1 years and median salary £1,843, while group 11 (Market Research) contains 42 persons of median age 36·7 years and median salary £1,775.

TABLE 15

Salary and Industry

Salary (£)	Industrial Groups 1	2	3	4	5	6	7	8	9	10	11
0–	5	5	2	2	3	0	3	6	23	4	1
750–	2	8	6	12	10	9	5	8	18	2	5
950–	11	14	9	12	7	8	4	8	23	5	4
1,150–	11	29	5	14	11	12	5	8	36	8	5
1,450–	18	43	7	17	10	13	13	7	36	4	11
2,000–	5	33	5	16	6	14	8	3	27	1	16
No reply	1	—	—	—	—	2	—	1	—	—	—
Total	53	132	34	73	47	58	38	41	163	24	42
Median salary	£1,395	£1,578	£1,180	£1,386	£1,259	£1,852	£1,556	£1,413	£1,300	£1,206	£1,775
Median age	35·7	40·9	44·1	42·8	35·1	44·8	41·3	33·5	38·7	41·6	36·7

Salary (£)	Industrial Groups 12	13	14	15	16	17	18	19	20	21	22
0–	6	2	1	4	4	4	4	0	1	4	3
750–	3	3	5	8	9	4	7	5	6	3	1
960–	11	2	11	4	16	2	6	8	3	5	0
1,150–	11	2	8	8	27	7	1	7	3	3	0
1,450–	21	4	6	11	24	7	8	5	6	4	4
2,000–	74	9	7	3	13	15	7	10	10	2	4
No reply	8	—	—	—	—	4	1	2	—	2	2
Total	134	22	38	38	94	43	34	37	29	23	14
Median salary	Over £2,000	£1,794	£1,244	£1,281	£1,356	£1,843	£1,150	£1,407	£1,633	£1,250	—
Median age	44·9	46·9	37·0	34·5	35·8	38·1	35·8	35·6	38·5	46·2	—

TABLE 16

Salary and Employment Category

Salary (£)	*I Civil Service*	*II Public Utilities etc.*	*III Research Bodies*	*IV Universities*	*V Finance*	*VI Mnfg. Industry*	*VII Miscellaneous*
0–	5·4	4·5	6·6	14·1	4·5	5·8	18·0
750–	5·4	18·2	16·1	11·0	5·6	14·1	9·8
950–	13·5	18·2	14·6	14·1	8·6	16·0	16·4
1,150–	21·6	19·5	18·2	22·1	9·1	19·5	18·0
1,450–	33·0	22·1	24·1	22·1	18·2	21·4	19·7
2,000–	20·5	17·5	18·2	16·6	50·0	20·8	11·5
No reply	0·5		2·2		4·0	2·6	6·6
Total percentage	99·9	100·0	100·0	100·0	100·0	100·2	100·0
Total No.	185	154	137	163	198	313	61
Median salary	£1,522	£1,295	£1,366	£1,300	Over £2,000	£1,371	£1,259
Median age	42·4	39·6	39·4	38·7	42·6	36·1	44·0

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The large group of 163 persons in universities has a median age of 38·7 years and a median salary of £1,300. The cost of living increase which was awarded to this group in August, 1957 makes these figures somewhat unsuitable for comparative purposes.

The most badly paid groups would appear to be group 3 in which 73 persons in Local Government Service of median age 44·1 years have a median salary of £1,180 and group 10 of non-university teachers in which 24 persons of median age 41·6 years have a median salary £1,206.

From Table 16 we see that, apart from the Miscellaneous group, category V (Finance) has the highest median salary (over £2,000) and the highest median age (42·6 years). Manufacturing Industry has a median salary (£1,371) higher than all other categories except Finance and the Civil Service, although it has the lowest median age of 36·1 years.

The most highly paid category of women were those in the Civil Service, who had also a high median age.

4.3. *Salary by Type of Work*

Table 17 shows the distribution of respondents in salary ranges for each of the seven occupational groups. The median salary for each group is also shown and the highest median is seen to be for those doing administrative work. The actuarial group comes next with a median salary of £1,911, but it must be remembered that apart from the Miscellaneous group these groups have the highest median ages of 44·7 and 46·2 years respectively. The lowest medians are to be found in the statistical and operational research groups, but these groups have median ages of 36·4 and 33·9 years.

TABLE 17

Salary and Type of Work

Salary (£)	*Type of Work*							
	Statistical (%)	*Scientific* (%)	*Economic* (%)	*Administrative* (%)	*Actuarial* (%)	*Op. Research* (%)	*Teaching* (%)	*Miscellaneous* (%)
0–	8·0	11·5	7·5	3·5	3·5	5·0	11·1	22·2
750–	18·1	9·6	8·4	5·2	5·6	13·3	6·7	11·1
950–	17·7	14·0	11·2	8·3	8·4	25·0	24·4	11·1
1,150–	22·8	24·2	18·7	8·3	8·4	26·7	24·4	11·1
1,450–	21·7	21·7	32·7	23·6	21·7	21·7	24·4	16·7
2,000–	10·8	17·2	20·6	49·8	46·9	6·7	6·7	11·1
No reply	0·9	1·9	0·9	1·3	5·6	1·7	2·2	16·7
Total	100·0	100·1	100·0	100·0	100·1	100·1	99·9	100·0
Total No.	452	157	107	229	143	60	45	18
Median salary	£1,227	£1,328	£1,521	Over £2,000	£1,911	£1,225	£1,245	£1,150

5. USE OF STATISTICAL TECHNIQUES

75·1 per cent. of respondents replied to Question 10 of the questionnaire in the affirmative. Thus although only 37·3 per cent. were employed primarily as statisticians, a further 37·8 per cent. did require some knowledge of statistical techniques. 24·9 per cent. of respondents (12·7 per cent. of women) did not require, in either their work or research, any knowledge of statistical techniques.

It is of some interest to consider the ways in which these two categories of persons

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differed, although the characteristics of those requiring techniques will be dealt with in more detail in a further paper.

First of all, in regard to age, the users of techniques, Group A, were considerably younger on the average, than the non-users, Group B, having a median age of 37·6 as compared with 44·1. The distribution of Group B by type of work shows the largest category, 38·2 per cent., to be persons doing administrative work, while 28·2 per cent. were doing actuarial work. Eighteen persons classified as doing statistical work were in Group B. They were presumably handling data without using techniques, or acting principally in an administrative capacity.

Looking at Group A, we find among the users of techniques 129 scientific and medical workers, 93 economists, 113 managers, 58 actuaries and 55 operational research workers.

A distribution by industry showed that of the 22 industrial groups those containing the greatest proportion of persons using techniques were the scientific research workers (Group 8) and those in the Scientific Civil Service (Group 1). Groups with the lowest proportion of users of techniques were the Miscellaneous group (21), the Actuaries (Group 12) and the workers in the Retail and Wholesale Distributive Trades (Group 13). Table 18 shows the distribution by industrial categories for Group A and Group B.

TABLE 18

Use of Techniques and Employment Category

	Employment Category							
	I	*II*	*III*	*IV*	*V*	*VI*	*VII*	*Total*
A. Users . . .	158	113	110	134	111	242	42	910
B. Non-users . . .	27	41	27	29	87	71	19	301

In Table 19 the class of the First degree held is shown separately for persons in Groups A and B. As can be seen, Group A contains a higher proportion of persons with a second class degree and of those with no degree, while B contains a remarkably high proportion of firsts and also a high proportion of those with a third or pass degree.

The median salary for Group A, the users of techniques, was £1,362 and for Group B, the older, non-users, was £1,701.

TABLE 19

Use of Technique and Class of Degree

	Class of Degree					
	First	*Second*	*Third*	*Pass*	*None or No Reply*	*Total*
	(%)	(%)	(%)	(%)	(%)	(%)
A. Users . . .	23·5	38·0	5·9	10·9	21·8	100·1
B. Non-users . . .	42·5	9·3	16·6	27·9	3·7	100·0
	(%)	(%)	(%)	(%)	(%)	(%)

6. SUMMARY

1,138 Fellows of the Royal Statistical Society and 73 Fellows or Associates of the Association of Incorporated Statisticians completed the questionnaires from which the results presented in this paper were obtained. They represent 55·1 per cent. of those to

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whom questionnaires were sent. 17·1 per cent. replied without completing a questionnaire, stating that they were not doing statistical work, or had retired, and the remaining 27·8 per cent. did not reply.

Of the 1,211 persons, 71 of whom were women, whose completed questionnaires were received, 75 per cent. sometimes used statistical techniques, but only 37 per cent were employed as statisticians. The other users of techniques were scientists, economists and business men. The work done and the techniques used by these persons will be discussed in detail in a second paper.

The group of non-users of techniques, more than 50 per cent. of whom were employed in industry or commerce, consisted primarily of persons doing work of an actuarial, financial or administrative nature. They were older on the average and better paid than the users of techniques. As many as 42·5 per cent. had first class degrees and only 3·7 per cent. had no degree but many had a third class or a pass.

Just over 40 per cent. of all respondents were employed in industry and commerce, and 15 per cent. were in the Civil Service. 35·5 per cent. had first class degrees and 26·9 per cent. had no degree. The median salary was £1,430 and the median age 39 years. Within each age range women appeared to be paid noticeably less than men, the difference being greatest in the range 40–49 years in which many men, but no women, had over £2,000. This difference reflects not so much a sex differential, however, as the absence of women among the highly paid actuarial group.

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7. APPENDIX

The Questionnaire

CONFIDENTIAL RESEARCH TECHNIQUES UNIT Serial No.

Enquiry on the Utilisation of Statistical Methods

Please answer the following questions, either in full in the space provided, or by placing a ring round the appropriate number.

SECTION I

1. Age next birthday.....years.

2. Place of birth: County .....

Country .....

3. Give full details of your *present* paid employment. Where more than one, give details of that which occupies the largest proportion of your time.

(*a*) Name of firm, organisation, etc.

.....

(*b*) Nature of your employer's business or profession.

.....

(*c*) Your present position or status.

.....

(*d*) Section or Department in which you worked.

.....

(*e*) Scope of your activities.

(*f*) Employed here from 19..... to 19.....

4. Give full details of the last job you held before your present one.

(*a*) Name of firm, organisation, etc.

.....

(*b*) Nature of your employer's business or profession.

.....

(*c*) Your position or status.

.....

(*d*) Section or Department in which you worked.

.....

(*e*) Scope of your activities.

) Employed here from 19..... to 19.....

5. If your *present* job is *non-statistical* in nature give details of your last *statistical* job, if different from that described in Q.4.

If your *present* job is *statistical* give details of your last *non-statistical* job if different from that described in Q.4.

(*a*) Name of firm, organisation, etc.

.....

(*b*) Nature of your employer's business or profession.

.....

(*c*) Your position or status.

.....

(*d*) Section or Department in which you worked.

.....

(*e*) Scope of your activities.

(*f*) Employed here from 19..... to 19.....

6. Are your duties in your present job *principally* concerned with organisation and management?

Yes 1
No 2

7. (*a*) Is your salary:

Under £750	Y
£750 and under £950	X
£950 and under £1150	0
£1150 and under £1450	1
£1450 and under £2000	2
£2000 or more?	3

(*b*) What additional benefits do you have? e.g. bonus, free lunch, non-contributory pension.

(*c*) Was your additional earned income in the past year:

Under £50	Y
£50 and under £150	X
£150 and under £250	0
£250 and under £500	1
£500 and under £1000	2
£1000 or more?	3

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APPENDIX—*contd.*

8. Give below full details of any first degrees you have obtained. ☐☐☐☐☐

(*a*) *University* (*b*) *College*	*Degree*	*Main subject*	*Year taken*	*Class obtained*	*Full time*	*Part time*
(*a*) (*b*)					1	2
(*a*) (*b*)					1	2

9. Give details of any diplomas or *higher* degrees which you have obtained or for which you are now working:

Year taken	*Diploma, Degree, etc.*	
.....	.....	☐
.....	.....	☐
.....	.....	☐

10. Do you in your work or research require knowledge of any statistical techniques?

Yes 1
No 2

IF "YES" TO Q.10 ANSWER SECTION II
IF "NO" DO NOT PROCEED FURTHER

SECTION II

11. (*a*) Have you held your present position:

Under 3 months	Y
3 months and under 1 year	X
1 years and under 5 years	0
5 years and under 10 years	1
10 years or more?	2

(*b*) Are you the first person to hold this position?

Yes 1
No 2

12. What is the total number of years for which you have been engaged on work of a statistical nature?

0– 1	Y
2– 5	X
6–10	0
11–15	1
16–20	2
Over 20	3

13. Indicate the frequency with which you apply or supervise the application of the following techniques:

Technique	*Frequently*	*Quite often*	*Occasionally*	*Never*
Sampling design	1	2	3	4
Design of Experiments	1	2	3	4
Analysis of Variance	1	2	3	4
Tests of Significance:				
"t"	1	2	3	4
Standard error	1	2	3	4
χ^2 (goodness of fit)	1	2	3	4
χ^2 (independence)	1	2	3	4
"F"	1	2	3	4
Non-parametric methods	1	2	3	4
Correlation	1	2	3	4
Ranking methods	1	2	3	4
Regression	1	2	3	4
Factor Analysis	1	2	3	4
Discriminatory Analysis	1	2	3	4
Curve Fitting (non-graphic)	1	2	3	4
Index Numbers	1	2	3	4
Time Series:				
Moving averages	1	2	3	4
Seasonal variation	1	2	3	4
Correlogram analysis	1	2	3	4
Sequential methods	1	2	3	4
Quality control	1	2	3	4

14. Which other techniques do you use frequently or quite often?

15. For those techniques listed in Q. 13 and Q. 14 give details of principal fields of application. Be as specific as possible.

Technique	*Field of Application*	
		☐
		☐
		☐
		☐

16. To which three books on statistical theory do you most frequently refer?

☐
☐
☐

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APPENDIX—*contd.*

17. Describe the work you did in your last complete working day, estimating the time spent on the various types of work.

18. Do your official duties involve:

(*a*) the development of new techniques

Yes 1
No 2

or

(*b*) the application of known techniques in completely new fields?

Yes 1
No 2

19. Are you at present engaged in any specific research projects in statistics?

Yes 1
No 2

20. How many of these projects are:

your own work .....

or the work of a

team or department? .....

Not applicable ..... Y

21. In which field of applied statistics or statistical theory are you most interested in doing research at the moment?

22. Indicate which of the following journals you see regularly, and the approximate frequency with which you read articles in them.

Journal	*SEE REGULARLY*	*READ*		
		One or more articles per issue	*About one article per yr.*	*Less than one article per yr.*
R.S.S. A	Y	1	2	3
R.S.S. B	Y	1	2	3
Applied Statistics . .	Y	1	2	3
The Incorporated Statistician	Y	1	2	3
J.A.S.A.	Y	1	2	3
Biometrics . . .	Y	1	2	3
Econometrica . . .	Y	1	2	3
Annals of Math. Stats. .	Y	1	2	3
Biometrika . . .	Y	1	2	3

23. Which other journals connected with your work or research do you read regularly?

24. (*a*) List with full details of date and place of publication the last three books or articles you have had published or accepted for publication.

(*b*) How many articles and books have you had published since January, 1955?

Articles .....

Books .....

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APPENDIX—*contd.*

25. Do you feel that any of the following seriously discourage or impede your research?

Lack of money	Y
Lack of time	X
Lack of references	0
Difficulty in discussing with colleagues...	1
Time taken to publish	2

26. Which meetings of the R.S.S. or the A.I.S. have you attended in the past year?

☐ ☐ ☐ ☐ ☐

THE QUESTIONS IN SECTION III SHOULD BE ANSWERED ONLY BY THOSE TEACHING OR DOING RESEARCH IN SCHOOL, COLLEGE OR UNIVERSITY

SECTION III

27. For how many hours per week do you lecture in statistics?

..... ☐

And in other subjects? ☐

.....

28. (*a*) When was statistics first taught in your institution?

..... ☐

(*b*) How many persons are now lecturing in the subject?

..... ☐

This enquiry is being carried out by the Research Techniques Unit.

Please complete the questionnaire and return it as soon as possible in the stamped addressed envelope which is provided.

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ANNUAL REPORT OF THE COUNCIL

For the FINANCIAL YEAR *ended December 31st, 1957, and for the* SESSIONAL YEAR *ended June 18th, 1958, presented at the* ONE HUNDRED AND TWENTY-FOURTH ANNUAL GENERAL MEETING *of the* ROYAL STATISTICAL SOCIETY, *held at the London School of Hygiene and Tropical Medicine, W.C.1, on June 18th, 1958.*

Number of Fellows

THE calendar year 1957 was again one of growth in the number of Fellows, the total on December 31st being 2,889. Of these 107 were representatives of corporate bodies. The net gain for the year was 117. The number of Fellows elected or restored to the roll during the year was 226. Losses by death, resignation or default totalled 109.

Further elections during the early months of 1958 brought the total number of Fellows at the end of April to 2,928.

The number of Honorary Fellows at the end of 1957 was 32, of whom 7 were Presidents of other Societies.

	Number of Fellows (excluding Honorary Fellows)			
	Losses			
Calendar Year	*By Death*	*By Resignation or Default*	*Elected or Restored to the Roll*	*On the Roll at December 31st*
1946	24	32	287	1,648
1947	14	61	263	1,836
1948	17	79	238	1,978
1949	16	87	189	2,064
1950	10	83	222	2,193
1951	21	93	228	2,307
1952	15	89	221	2,424
1953	10	91	165	2,488
1954	14	106	171	2,539
1955	19	83	182	2,619
1956	16	85	254	2,772
1957	11	98	226	2,889

Losses by Death

The Council records with regret the loss during the year ended April 30th, 1958, of the undermentioned Fellows:

	Date of Election
Zwiedineck-Südenhorst, Professor O. von (Honorary Fellow)	1954
Bird, Leslie, E.	1935
*Clow, Sir Andrew G., K.C.S.I., C.I.E., M.A.	1921
*cp**Glenday, Roy G., M.C.	1921
Grace, Arthur Daniel	1945
Lewsley, Bernard John, B.Sc.	1944
Lockyer, Ivan Joseph, B.Sc.	1947
Menken, Jules	1925
Munshi, T. N.	1922
Norris, Vera, M.B., Ch.B., Ph.D.	1952
cp Paish, Sir George	1899
p Rushton, Stanley, M.A., B.Sc., Ph.D., D.I.C.	1947
Swain, Joseph Frank, B.Sc.	1951
Weiss, Samuel, A.B., M.A. (d. 1955)	1953
c Womersley, John Ronald, B.Sc., D.I.C.	1942

* Life Fellow. *c* Served on Council. *p* Contributed to *Proceedings*.

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Professor O. von Zwiedineck-Südenhorst was elected an Honorary Fellow in 1954. He had been Professor of Economics and Statistics and Director of the Institute of Statistics in the University of Munich since 1921. He had an international reputation as a statistician and was an Honorary Fellow of the International Statistical Institute.

Sir Andrew G. Clow, the last British Governor of Assam, was a Life Fellow, his election dating back to 1921. His outstanding work was the improvement of labour conditions throughout undivided India. This began with his appointment in 1920 as Controller of the Labour Bureau of the Government of India, after six years' service in the Indian Civil Service. During the next eighteen years he was engaged in ameliorative work and legislation, being successively Deputy Secretary of the Industries and Labour Department, Deputy Joint Secretary in 1931 and Secretary from 1936. From 1929 to 1931 he was also a member of the Royal Commission on Indian Labour, drafting the greater part of that Commission's report. In 1938 he took a seat on the Viceroy's Executive Council as Member for Communications and held this post till the middle of the 1939–45 war. It was in 1942 that he became Governor of Assam, and in 1946 he acted for some months as Governor of Bombay. Internationally he was also a valued adviser and delegate, serving on five occasions on the annual International Labour Conference at Geneva, and representing the United Kingdom on the United Nations Organization Commission for the Far East in 1947 and 1948. His last appointment was at home in 1949, as Chairman of the Nationalized Gas Industry in the Scottish area.

Mr. Roy Glenday had served the Society as a member of Council for four periods covering fifteen years, and in 1943–45 he was a Vice-President. His consistent good-humour and unassuming manner did not prevent him from expressing criticism when he considered statistical analysis to be misdirected, and one of the three papers he read to the Society was entitled "The use and misuse of economic statistics". His colleagues on the Council will long remember how easy he was to work with, and how valuable were the wide knowledge of industry and the good judgment he brought to bear on the Society's affairs. He was widely known for the pioneer work he had done as Economic Adviser to the Federation of British Industries, a post he held from the end of the 1914–18 war to 1954. In this capacity he represented British industry at many conferences in Europe and elsewhere.

Mr. Jules Menken, a Fellow from 1925 onwards, was an economist and politician rather than a statistician, and although he joined in discussion at the Society's meetings, he never contributed to the *Journal* or served on the Council. After taking a double first in Economics at Cambridge, he became a member of Lloyd George's secretariat and carried out much of the economic research behind the "Yellow Book". Later, following a period as economic adviser to a London department store, he spent five years as head of the Department of Business Administration in the London School of Economics. It is for his writings on military affairs and the politics of defence that he gained a reputation which was strong in America as well as in this country.

Sir George Paish, who was 89 when he died, became a Fellow as long ago as 1899, when he was assistant editor of the *Statist*. He was five times a member of Council, between 1907 and 1917, and contributed to the *Journal* two papers on Great Britain's capital investments abroad, one on war finance and one (with W. M. Acworth) on British railway accounts and statistics, as well as a series on commodity prices in 1913–15. His association with the *Statist* was a lengthy one, for he became secretary to the editor while

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still in his teens, was successively assistant editor and joint editor, and held the latter post till 1916. A recognized authority on the economic statistics of overseas trade and on the subject of Commonwealth loans, he was also a leader in the reform of railway statistics. From 1906 to 1908 he was a member of the Departmental Committee of the Board of Trade on Railway Accounts and Statistics. In 1914 he was chosen as adviser to the Chancellor of the Exchequer on financial and economic questions. His was a full life as well as a long one, for to his professional interests he added speaking and writing on the principles of Liberalism and of Free Trade.

Dr. S. Rushton was a member of the staff of the Mathematics Department at Imperial College for ten years. His research began with contributions to the theory of sequential tests, and he went on to make notable contributions to numerical analysis, in the solution of linear equations and the computation of the confluent hypergeometric function. At the time of his death he was publishing a joint paper with Dr. Neumann on meteorological time series; in this field he had arrived at, but not published, an important result concerned with tests of significance to be applied to filtered series. Dr. Rushton worked hard in the peace movement, and at one time edited the journal *Science for Peace*.

Mr. J. R. Womersley, elected a Fellow in 1942, served on the Council from 1945 to 1948. His experience in the application of statistical methods to industrial problems covered a wide field. Having assisted Dr. D. R. Hartree in the development of the Manchester differential analyser and worked at the Shirley Institute applying mathematical methods to textile problems, he joined the ballistics branch of the Research Department, Woolwich, at the beginning of the Second World War and there inaugurated a statistical investigation of cordite proof records. In 1942 he was appointed the first head of the Ministry of Supply's Advisory Service on quality control, and at the end of the war he became the first superintendent of the mathematics division of the National Physical Laboratory. From this he turned to the development of the Hollerith electronic computer, and subsequently was connected with computing work for the United States Air Force. Recently he collaborated with a team at St. Bartholomew's Hospital in a mathematical investigation of the flow of blood in the arteries. Alert in discovering new fields for statistical methods, he had also the gift of stimulating others to carry out work in these while he himself passed on to pioneer elsewhere.

Distinctions

It gave the Council much pleasure to convey its congratulations to the President on his receiving the honour of Knight Bachelorhood in the Birthday Honours. It was also with much pleasure that the Council extended its felicitations to Mr. H. Tetley on his award of the honour of Companion of the Bath, announced in the New Year's Honours List. On the retirement from public service of Sir George Maddex at the end of March, Mr. Tetley became the Government Actuary and the Council is glad that the traditional and close association between the occupant of that office and the Society thus continues.

Honorary Fellowship

There were elected to Honorary Fellowship at the Ordinary Meeting on June 19th, 1957, the following statisticians of world-wide reputation:

Miss Gertrude Cox
Professor Carlos E. Dieulefait
Dr. Robert C. Geary
Professor René Roy

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The Honorary Fellowship Committee has given further consideration to the Society's roll of Honorary Fellows and in the light of its recommendations the Council is proposing for election three more statisticians of international standing.

Vice-Presidents

The President appointed as Vice-Presidents of the Society for the Session 1957–58 Mr. R. F. George, Professor M. G. Kendall, Mr. F. A. A. Menzler and Mr. L. H. C. Tippett.

Meetings of the Society

It is in no merely formal sense that the Council again expresses its gratitude to the Dean and the Board of Management of the London School of Hygiene and Tropical Medicine for the use of their lecture theatre for the Ordinary General Meetings of the Society and for meetings of the Research Section. Accommodation of this kind is very scarce in London and the Society is particularly fortunate in its enjoyment of these facilities.

The papers read at the Ordinary General Meetings were as follows:

1957	
November 20th	Presidential Address. Recent Developments in Economic Statistics.
December 17th	J. S. CRAMER. The Depreciation and Mortality of Motor-cars.
1958	
January 13th	M. BRICHLER. The Classification of the Population by Social and Economic Characteristics.
February 19th	M. G. KENDALL. Research in Statistics.
March 19th	Sir ROBERT SHONE and H. R. FISHER. Industrial Production and Steel Consumption.
April 16th	A Discussion on Some Problems in the Measurement of Price Changes with Special Reference to the Cost of Living, opened by S. J. PRAIS.
May 14th	Sir KEITH MURRAY. The Development of the Universities in Great Britain.

Attendances again varied widely and ranged betewen 55 and 166.

The Research Section

Professor G. A. Barnard was elected Chairman of the Research Section for the Session and Mr. D. V. Lindley was re-elected Honorary Secretary. Other members of the Committee were: Mr. N. T. J. Bailey, Dr. D. E. Barton, Mr. E. M. L. Beale, Professor H. E. Daniels, Mr. M. J. R. Healy, Dr. C. A. B. Smith, Mr. A. Stuart, and Mr. A. M. Walker, with Professor M. S. Bartlett and Dr. D. R. Cox appointed by the Council. Dr. J. O. Irwin was an *ex officio* member of the Committee as Editor of the *Journal, Series B*. During the Session Mr. Beale resigned on going abroad and Dr. P. Armitage was appointed in his stead.

The papers read during the Session have been as follows:

1957	
December 18th	J. NEYMAN and ELIZABETH SCOTT. Statistical Approach to Cosmology.
1958	
January 22nd	G. S. WATSON. On Chi-Square Goodness-of-Fit Tests for Continuous Distributions.
March 5th	D. R. COX. Regression Analysis of Binary Sequences.
May 7th	W. L. SMITH. Renewal Theory and its Ramifications.

The Council, in addition to expressing its thanks to the Dean and the Board of Management of the London School of Hygiene and Tropical Medicine for the use of their Lecture

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Theatre for Research Section meetings, has also to acknowledge its indebtedness to the Provost of University College and to the Director of the London School of Economics for kindly providing accommodation for the December and January meetings respectively.

The Study Section

The Section Committee for the Session has been: Mr. B. P. Emmett (Chairman), Mr. G. C. C. Chivers (Honorary Secretary), Mr. B. Benjamin, Miss S. V. Cunliffe, Mr. J. S. Downham, Mr. J. Durbin, Mr. E. Shankleman and Mr. L. T. Wilkins. The Council representatives were Mr. Benjamin and Mr. Durbin. Mr. R. Savage represented the Bristol Group at Committee meetings.

The average attendance at meetings in London for the Session has been 46.

The first meeting of the Session on October 9th was a joint meeting with the Institute of Actuaries Students' Society. On this occasion Mr. C. T. Saunders addressed the Section on "Current Economic Analysis—New Plans for the National Institute of Economic and Social Research". The President of the Society, Sir Harry Campion, presided.

The following other papers were presented:

1957	
November 13th	A Practical Example of Linear Programming—Alison Doig.
December 11th	Economic Problems of Small and Medium-sized Firms—H. F. Lydall.
1958	
January 8th	The Measurement of Non-Industrial Work—H. W. G. Deeks.
February 12th	Discriminant Analysis and Perinatal Mortality—J. A. Heady.
March 12th	Planning and Organizing Budget Surveys—W. F. F. Kemsley.
April 9th	Economic and Statistical Problems of Airline Operations—S. F. Wheatcroft.
May 21st	Long Term Demands for Fuel—E. F. Schumacher.

The Study Section have held their meetings this year at the London School of Economics, and the Council expresses its thanks to the Director for so kindly providing this accommodation.

The Second Conference on Business and Economic Statistics was held at Queens' College, Cambridge, over the week-end March 21st–23rd, 1958. Professor R. G. D. Allen took the Chair at the Conference, and Sir Harry Campion gave an address at the Opening Dinner.

Over 180 people attended the Conference.

The following papers were read:

Official Labour Statistics in Great Britain and the Analysis of the Current Labour Scene—H. A. Turner.
Statistics of U.K. Income—G. Paine.
Consumer Expenditure Surveys: An Appraisal of Recent Work—Mark Abrams.
Developments in British Short-term Economic Statistics over the Last Few Years, and the Future—C. T. Saunders.
Measuring Industrial Development and its Impact on Imports of Manufactured Goods—A. Maizels.
Operational Research and the Business Statistician—J. Harling.
How Useful is the Decennial Census of Population to Business?—T. Corlett.
Financial Statistics and their Significance for Economic Forecasting—A. Luboff.

Business Outlook:
Introductory: The General Economic Position—W. B. Reddaway.
Household Durable Goods—Max Adler.
Textiles—J. A. Blackburn.
Outlook for the Motor-Car Industry—K. D. Bull.
Machine Tools—M. E. Beesley.
Building and Construction—Miss L. J. M. N. Milne.

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A limited number of copies of the Papers are available on sale, on application.

Bristol Group

The Committee of the Bristol Group for the Session has been: Mr. R. Savage (Chairman), Miss M. E. Holgate (Honorary Secretary), Mr. B. K. Burford, Mr. H. C. Mackenzie, and Mr. R. J. Pounce.

Six meetings have been held, and the following papers heard and discussed:

1957	
October 17th	The Assessment of Therapeutic and Prophylactic Procedures—J. Knowelden.
November 21st	Computing Methods for Optimization and Problems Arising in Production Planning—C. M. Berners-Lee.
December 12th	The Full-Employment Level—R. J. Pounce.
1958	
January 16th	Motivation Research—D. H. Pickard.
February 13th	Election Forecasting—D. E. Butler.
March 20th	The Planning of Experiments—D. R. Cox.

Attendances (ranging from 10 to 36) have averaged 20.

The Council makes grateful acknowledgment to the University of Bristol for providing a room in which to hold the meetings and for the service of tea.

The Medical Section

The Committee of the Section during the Session has been: Dr. D. D. Reid (Chairman), Mr. B. B. Bonner (Honorary Secretary), Mr. M. P. Curwen, Dr. W. R. S. Doll, Mr. J. A. Heady, Mr. D. Hewitt, Mr. S. Rosenbaum, and Dr. C. C. Spicer.

The following meetings were held:

1957	
October 8th	Social Aspects of Prescribing—J. P. Martin.
October 29th	Problems in the Collection and Analysis of Morbidity Data—Ann Cartwright.
November 26th	Observer Variability in the Use of Respiratory Questionnaires—C. H. Wood: Design and Conduct of Study; A. S. Fairbairn: Analysis of Results.
1958	
January 28th	Mortality of Miners—D. D. Reid.
February 25th	Pulheems—S. Rosenbaum.
March 25th	Methodological Problems in the Study of Occupational Mortality—M. A. Heasman.
April 29th	Some Current Problems in Demography—P. R. Cox.
May 27th	Mortality from Lung Cancer in Relation to Population Density—R. E. Waller.

Attendances have averaged 31.

The Council again expresses its thanks to the Dean of the Westminster Medical School for providing accommodation for the meetings.

The Industrial Applications Section

The Section Committee which co-ordinates the functions of the separately organized Local Groups and links them with the Society has consisted of: (*a*) appointed by the Council: Professor E. S. Pearson and Mr. E. C. Fieller; (*b*) nominated by the 1956–57 Section Committee: Dr. C. J. Anson, Mr. A. Stafford Beer, Mr. D. J. Page, Mr. E. D. van Rest, Mr. G. W. Sears, Mr. A. Soper, Mr. A. F. Tout, Mr. M. Whyte; (*c*) representing Local Groups: Mr. M. A. Aczel, Mr. A. D. Airth, Mr. M. G. Dixon, Mr. W. A. Hay,

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Mr. I. F. Hendry, Mr. J. H. Hepburn, Mr. P. M. Hooper, Mr. D. A. Hopkins, Mr. A. N. James, Mr. B. J. A. Martin, Mr. W. R. Matheson, Mr. P. D. Oldham, Mr. A. F. Purser, Mr. D. Sirkin, Mr. J. P. L. Truesdale.

The Section Committee re-elected Mr. G. W. Sears as Chairman, and Mr. D. J. Page was elected Honorary Secretary.

The Officers of the Local Groups were:

Group	*Chairman*	*Honorary Secretary*
Birmingham and District	Mr. B. J. A. Martin	Mr. W. N. Jessop
Edinburgh	Mr. A. F. Purser	Mr. D. H. S. Forbes
Glasgow	Mr. J. V. Gregg	Mr. W. R. Matheson
Leicester	Mr. J. H. Simcock	Mr. P. M. Hooper
London	Mr. I. F. Hendry	Mr. A. N. James
Merseyside	Mr. J. P. L. Truesdale	Mr. A. Muir
North-Eastern	Mr. D. Newman	Mr. M. G. Dixon
Sheffield	Mr. D. A. Hopkins	Mr. M. A. Aczel
South Wales	Mr. P. D. Oldham	Mr. W. A. Hay

Group membership on December 31st, 1957, was as follows (comparative figures for 1956 are shown in brackets):

	Fellows	*Non-Fellows*	*Total*	*Average Attendance*
Birmingham	41 (42)	23 (20)	64 (62)	22 (25)
Edinburgh	6 (6)	15 (17)	21 (23)	16 (20)
Glasgow	8 (8)	28 (28)	36 (36)	14 (15)
Leicester	16 (16)	24 (27)	40 (43)	18 (29)
London	176 (147)	96 (83)	272 (230)	50 (46)
Merseyside	17 (15)	30 (24)	47 (39)	17 (17)
North-Eastern	30 (36)	6 (6)	36 (42)	20 (22)
Sheffield	35 (35)	41 (34)	76 (69)	27 (21)
South Wales	12 (20)	21 (18)	33 (38)	27 (24)
	341 (325)	284 (257)	625 (582)	

Fellows who are recorded as members of the Section are those who signify their wish to be attached to a particular Group for the purpose of voting in its elections and being eligible to serve on its Committee.

The accounts of the Section and its Local Groups for the calendar year 1957, excluding expenditure by the Society in relation to the work of the Section, are summarized below:

Expenditure	£ s. d.	£ s. d.
Birmingham	33 18 5	
Edinburgh	10 1 2	
Glasgow	32 16 8	
Leicester	14 7 4	
London	27 4 9	
Merseyside	21 18 5	
North-Eastern	27 4 6	
Sheffield	21 4 3	
South Wales	7 1 2	
Section Committee	34 18 1	
		230 14 9
Carried forward to 1958		157 19 11
		£388 14 8

Income	£ s. d.
Brought forward from 1956	106 14 8
Grants from Council*	282 0 0
	£388 14 8

* Excluding expenditure incurred at the Society's offices.

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The total expenditure of £230 14*s.* 9*d.* compares with £245 5*s.* 1*d.* in 1956.

One-day conferences were held during the Session by the London and Birmingham Groups, both of which were well attended. The subject of the London Group Conference, which was held on December 13th, was "Production Control" and the speakers were Mr. Stafford Beer, Mr. C. Berners Lee and Mr. A. Battersby. The subject of the Birmingham Group Conference, held on Wednesday, May 7th, was "Statistical Sampling in Industrial Management".

Fifty-five meetings, listed below, were held by local Groups:

Birmingham and District Group

1957	
October 2nd	A Sampling Inspection Scheme and its Consequences—R. H. S. Lesser.
October 30th	Simulation Methods—J. Harling.
December 4th	Statistical Inference—D. V. Lindley.
1958	
January 8th	Non-Linear Estimation—G. A. Coutie.
February 5th	Ranks and Variate-Values—M. G. Kendall.
March 5th	Random Walk Problems—L. S. Goddard.
March 19th	(Joint Meeting with the Birmingham Productivity Association): Comparison of Industrial Efficiency—L. H. C. Tippett and J. C. Pritchard.
April 2nd	The Planning of Experiments—D. R. Cox.

Edinburgh Group

1957	
October 22nd	Using Statistics to make Decisions—G. A. Barnard.
November 20th	The Application of Statistics to Screening Problems in the Pharmaceutical Industry O. L. Davies.
December 10th	Printers' Errors—A. Lord.
1958	
January 14th	(Joint Meeting with the Industrial Panel of the Scottish Film Council): Mr. A. Stafford Beer's Talkie-strip on Introducing Operational Research.
February 19th	Some Methods for Reducing Uncertainty concerning the Future—J. V. Gregg.
March 12th	Market Research—J. F. Hayes.
April 23rd	Errors in Choosing an Elite—Barnet Woolf.

Glasgow Group

1957	
October 23rd	Statistical Decision Theory—G. A. Barnard.
1958	
January 21st	Monte Carlo Methods—W. N. Jessop.
March 18th	Statistical Methods Applied to Attitude Studies—N. J. Squirrell.

Leicester Group

1957	
October 3rd	The Statistician and Personnel Management—B. Ungerson.
November 7th	Shapes and Sizes of Women's Feet—J. R. Manning.
December 5th	The Auditor and How He Works—P. N. Wallis.
1958	
January 30th	Smoking and Cancer of the Lung—W. R. S. Doll.
February 27th	Machine Interference—F. Benson.
March 27th	Methods of Productivity Measurements—T. E. Easterfield.

London Group

1957	
October 4th	Profits and Statistics—R. Brech.
November 1st	A Reliability Study of Sensory Assessments—A. S. C. Ehrenberg and J. M. Shewan.
December 13th	(One-Day Conference on Production Control.)

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1958

February 7th — Statistics in Management and Administration—L. H. C. Tippett.
March 7th — A Review of Some Sampling Inspection Schemes—I. D. Hill.
April 10th — Sophisticated Chemistry—J. Youden.
May 2nd — Component Replacement—J. Taylor.

Merseyside Group

1957

October 8th — Decisions from Samples—J. Ogilvie and J. P. L. Truesdale.
November 12th — Jump and Wiggle Analysis for Time Series—G. H. Jowett.
December 10th — Time and Methods Study—W. Jones.

1958

February 11th — Operations Research in the U.K. and U.S.—B. H. P. Rivett.
March 11th — Management Accounting—W. J. Jennett.
April 15th — Stochastic Linear Programming—S. Vajda.

North-Eastern Group

1957

October 9th — The Determination of Environmental Hazards—J. H. Edwards.
November 13th — Productivity Measurement at the Plant Level—T. E. Easterfield.
December 11th — Stock Control and Stock-taking, especially Bulk Stocks of Liquids and Solids—E. S. Milner.

1958

January 15th — The Effects of Television—Research Methods and Findings—W. A. Belson.
February 12th — Colliery Accidents and Statistical Design—F. D. K. Liddell.
March 12th — Some Applications of Statistics to Research on Animal Nutrition—A. W. Boyne.

Sheffield Group

1957

October 24th — Different Methods of Introduction and Organization of Quality Control in Manufacture involving Complex Processes—E. A. G. Knowles.
November 28th — Some Problems of Long-term Research in Industrial Medicine—J. R. Ashford.

1958

January 30th — The Installation of a Quality Control System—D. C. Webb.
February 27th — Unfair Queues—F. Downton.
March 27th — Automatic Sales Forecasting—A. Muir.
April 24th — Monte Carlo Methods—K. D. Tocher.

South Wales Group

1957

October 14th — The Epidemiological Approach to Medical Problems—I. T. T. Higgins.
November 11th — Discrimination in Physical Anthropology—M. J. R. Healy.
December 10th — Sequential Inspection and Control of Continuous Processes—W. D. Ewan and K. W. Kemp.

1958

January 13th — Simple Experimental Designs—D. R. Cox.
February 10th — The Design of Screening Tests in the Pharmaceutical Industry—O. L. Davies.
March 10th — The Use of a Computer in a Monte Carlo Synthesis—R. Neate.

The Council makes grateful acknowledgment to those bodies which have kindly provided accommodation for the meetings of the various Groups throughout the Session.

Award of the Guy Medal in Silver

The Council is pleased to award the Guy Medal in Silver to Professor G. A. Barnard whose original work in the development of statistical techniques is widely recognized. His study on Sequential Tests in Industrial Statistics, published in the *Supplement* in 1946, made a notable advance in methodology and subsequent papers, e.g. on Statistical

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Inference (Series B, 1949) and on the Theory of Information (Series B, 1951), were of no less outstanding importance. His interest in, and work for, the Research Section has largely contributed to the success of that branch of the Society's activities.

Frances Wood Memorial Prize

Seven entries were received for the Frances Wood Memorial Prize competition held in 1957. The prize, which was closely contested, was awarded to Mr. David Hewitt for his essay on "Mortality in the London Boroughs 1950–1952, with Special Reference to Respiratory Diseases". While congratulating Mr. Hewitt on his success, the Council at the same time records its thanks to Mr. Fieller, Dr. Reid and Dr. Barna who judged the entries.

The Society's Examinations

During the Session the Council decided that in view of the development of tuition courses and examinations in statistics which did not exist when the Society's examinations were inaugurated these examinations should be discontinued. The examination held in March was the last examination for the Certificate open to candidates entering for the first time. The Council will hold a special examination for the Certificate in March 1959 for those candidates who passed in two or more papers in 1958 or previously. The last examination for the Diploma will also be held in March 1959. The Council emphasizes that Certificates and Diplomas already awarded remain entirely unaffected by these decisions.

Fifty-one candidates for all or part of the Certificate and one for the Diploma entered for the examinations held on March 19th, 20th and 21st, 1958.

Certificates have been awarded to the following Fellows:

Airth, Alan Don
Coore, Haldane Gladstone
Coultate, John Henderson
Davis, John Roberts
Flowerdew, Anthony David John
Gore, Arthur Thomas
Jarvis, Julian Louis
Leaver, Rex Anthony
Lillistone, Janet Elizabeth
Mann, Paul Henri
Proctor, Jean Rosemary
Sharp, Donald Joseph
Tate, Elizabeth Anne Markham
Thomas, David Anthony Hayden
Thorneycroft, William Terence
Wheller, Barbara Elizabeth Anne
Whyte, Eric John
Winterford, Charles William
Young, Herbert Timothy

This year the examinations in London were held at the London School of Economics and Political Science and the Council expresses its thanks to the Director of the School for granting this facility. The Certificate examination was also held in the College of Technology, Birmingham, and in Dominica, British West Indies, and the Council is indebted to Dr. C. J. Anson (Birmingham) and Mr. O. E. Walker (Education Officer, Dominica) for accepting responsibility for the necessary local arrangements. It is also grateful to those who acted as invigilators and to the Examinations Committee which consisted of Mr. J. Durbin (Chairman), Professor R. G. D. Allen, Mr. N. T. J. Bailey, Mr. B. Benjamin, Mr. W. J. Corlett, Dr. D. R. Cox, Mr. B. P. Emmett, Professor H. E. Daniels, Dr. F. G. Foster, Mr. W. N. Jessop, Dr. N. L. Johnson, Miss R. J. Maurice, Mr. F. A. A. Menzler, and Dr. P. G. Moore.

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The Journals

In its Report last year, the Council referred to measures taken to make the *Journals* of the Society more attractive in appearance and presentation. The Council hopes that the changes introduced with the first part of the 1958 volume will meet with the approval of Fellows and other readers.

In spite of continued endeavour to avoid an increase in size, the *Journal* Series A for 1957 expanded to 534 pages—26 more than in 1956. An increase in the *Journal* Series B was recognized in the Session 1956–57 as inevitable and, as reported a year ago, the Council agreed that the volume for 1957 should contain about 350 pages. The careful attention of the Editors to this decision is evident in the actual size of the volume, which was 354 pages.

There is still an accumulation of papers for Series B and because of the rate at which they are submitted and accepted the Council has agreed a recommendation of its Journal Committee that the 1958 volume be further increased to about 420 pages. If the income from sales fails to correspond with the inevitably greater cost, the Council recognizes that attention will have to be paid to the question of increasing the price to subscribers and other purchasers. Some experience of income and cost associated with a volume of 420 pages is clearly necessary before a considered judgement can be taken on this point.

Other recommendations of the Journal Committee accepted by the Council were that a number of past issues of Series A and Series B which were becoming scarce should be reprinted and that the print order of Series A and B should be increased to 4,500 and 3,200 copies per issue respectively. Within the past six years the order for Series A has thus expanded by one-third while that for Series B has almost doubled.

The Council gratefully acknowledges the work of the Editorial Panel for the *Journal* Series B which has consisted of: Dr. J. O. Irwin (Editor), Mr. N. T. J. Bailey and Dr. D. R. Cox (Associate Editors), Dr. P. Armitage, Professor G. A. Barnard, Professor M. S. Bartlett, Professor H. E. Daniels, Dr. O. L. Davies, Mr. E. C. Fieller, Mr. J. M. Hammersley, Dr. N. L. Johnson, Mr. D. G. Kendall, Professor M. G. Kendall, Mr. D. V. Lindley, Mr. R. L. Plackett, Dr. C. A. B. Smith, Dr. K. D. Tocher, Dr. S. Vajda and Dr. F. Yates.

Applied Statistics

In October 1957 the Council regretfully accepted the resignation of Mr. B. T. Ramm from his appointment as Assistant Editor of *Applied Statistics*. His place has been taken by Mr. H. S. Booker, while Dr. D. G. Beech has continued as Editor. Other members of the Applied Statistics Committee have been: Mr. A. Stafford Beer, Miss S. V. Cunliffe, Dr. O. L. Davies, Dr. W. R. S. Doll, Mr. B. P. Emmett, Mr. R. F. Fowler, Dr. J. O. Irwin, Dr. G. H. Jowett, Mr. P. D. Oldham, Mr. E. Shankleman, Mr. P. J. Stanley, Mr. L. H. C. Tippett, and Mr. L. T. Wilkins.

The 1957 volume of *Applied Statistics* contained 232 pages, the increased size being necessitated by an accumulation of articles held over from 1956. During 1957 there was a slight decrease in the supply of suitable articles.

The numbers of subscribers in the last three years are given in the following table:

	Fellows	*Non-Fellows*	*Total*
1955 . . .	467	992	1,459
1956 . . .	508	1,035	1,543
1957 . . .	494	1,079	1,573

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It is gratifying that there has been no general decrease in numbers as a result of the increased subscription rate. On the other hand, the drop in the number of subscribing Fellows is disappointing.

The Library

The Library Committee concentrated its attention during the year on finding a solution of the problem of storage space for the historical series of foreign official publications which are stored away from Bentinck Street. The collection was moved during the year from one temporary store to another—both made available through the kind help of Fellows. The problem occupied much of the time of the Librarian and his scrutiny of the volumes (preparing the way for a decision on what might be rejected) necessitated a partial closing of the Library during the month of January 1957.

The table, which presents an analysis of requests made by Fellows to borrow books during each of the years 1953–57 inclusive, shows that in 1957 more calls were made on the services of the Library than in any recent year. The analysis does not include books consulted for reference.

	1953	1954	1955	1956	1957
Number of Fellows who made requests to borrow books	343	339	380	450	492
Number of applications made	1,319	1,032	1,370	1,744	2,096
Number of volumes borrowed	2,110	1,509	2,339	2,832	3,724
Number of volumes "on loan" (unavailable at time of request)	185	181	292	329	510
Number of volumes not possessed by the Society at time of request	174	137	266	333	295
Total number of volumes asked for	2,469	1,827	2,897	3,494	4,529
Non-serial works added to the Library	516	695	930	719	957

A recent analysis reveals that 55 per cent. of the total volumes borrowed were non-serial publications (text-books and the like), 32 per cent. were serial publications (mainly scientific journals including offprints from such journals) and 13 per cent. were official reports. Although many Libraries hold journals and official reports that are in the Society's Library, few allow them to be taken away and many Fellows appreciate being able to borrow these, especially through the post. The number of volumes of all kinds borrowed and returned by post has increased considerably in recent years and unfortunately the Library Committee has had to consider the extent of the damage caused by a few Fellows who return books by post without giving them sufficient protection.

A list of serial publications currently received in the Library was published in the *Journal*, Series A, 1957, pp. 510–526.

Finance

Abstracts of the Honorary Treasurer's Accounts, viz. the Statement of Income and Expenditure for the year 1957 and the Balance Sheet as at 31st December 1957, together with the Auditor's Report thereon are in Appendices B and C.

Total ordinary expenditure fell from £16,362 in 1956 to £14,375 in 1957. Most items increased but owing to the issue of only one part of Series B in 1957 compared with three parts in 1956, total Publication and Distribution Expenses of the Society's Journals declined from £9,349 in 1956 to £6,759 in 1957. A substantial rise must be expected in 1958.

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Total income rose from £15,257 in 1956 to £16,295. There was a further rise in Fellowship subscriptions of £443 to £7,299. Receipts from sales of the journals increased by £420 to £6,906 and there was a surplus for the year on the publication of *Applied Statistics*. As a result of the year's operations, income exceeded ordinary expenditure by £1,920 but this was mainly due to the issue in the year of only one part instead of two of Series B, already referred to, just as the deficit of £1,105 in 1956 was mainly due to the issue of three parts of Series B in that year.

Following the setting up of the Investments Policy Committee in 1957 steps were taken in April of that year to carry out the decision of the Council that, as a general rule, of the funds of the Society, taken at their book value, 35 per cent. should be invested in preference shares and equities, 15 per cent. in debenture and loan stock and 50 per cent. in trustee securities.

The investments then held by the Society, all in the gilt-edged class, had a total book value of approximately £31,000 and there was also available for permanent investment a sum of £2,750. It was therefore decided to sell approximately £17,000 of the existing holdings and to re-invest the proceeds as to some £5,000 in Debenture and Loan stocks and as to £12,000 in Preference and Ordinary Shares. As a result of these operations the Society has gained an increase of approximately £100 per annum in its income from investments. A detailed list of the Society's holdings as at 31st December 1957 is given in Appendix D.

A necessary consequence of the new investment policy is that, in general, the Society's portfolio of investments should be regarded as a whole and not associated with particular funds. This is reflected in the Balance Sheet where, apart from the Investment of the Leasehold Sinking Fund, the Society's holdings of stocks and shares have been aggregated under the title "General Investments", amounting to £30,747 at cost.

The balance of £1,920 on the Statement of Income and Expenditure was added to the Accumulated Fund. This increase in the Accumulated Fund was however more than offset by two items of exceptional expenditure, viz., £1,911 representing a past service contribution to the staff superannuation scheme and £720 representing the book loss on sale of investments. As a result the Accumulated Fund stood at £11,078 at the end of 1957 compared with £11,653 at the end of 1956.

Life Composition Fees received during the year amounted to £332 and were added to the Life Composition Fee Fund. After transferring to the Income and Expenditure Account the fees of compounders who died during the year or were presumed to have died in previous years (£39) the Fund, which is maintained at the total of the Composition Fees received from Fellows still living, increased from £7,922 to £8,215.

The Council and Officers

It is with pleasure that the Council reports that Sir Harry Campion has accepted its cordial invitation to continue as President for the coming Session. His wide administrative experience and shrewd judgement have been of great service to the Council in the conduct of its business.

Mr. R. F. George retired from the post of Honorary Secretary at the end of the Session, having served in that capacity for the maximum period of ten consecutive Sessions allowed by Bye-law 13. Mr. George has worked hard and devotedly to further the interests of the Society during a period of post-war reconstruction which included the move to 21,

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Bentinck Street, a rise in the number of Fellows from just under 2,000 to almost 3,000, and a growth in the activity of the Society of which he may be justly proud. The Society will always be heavily in his debt for this alone though the account of his contribution to the Society is fortunately not yet closed.

The ordinary members of the Council who retire at the close of the current Session are: Miss J. Keen, Miss S. V. Cunliffe, Professor M. S. Bartlett, Professor R. Stone, Mr. J. Stafford and Mr. G. W. Sears. The Council gladly acknowledges its indebtedness to them for the services they have rendered. After observing the requirements of the Bye-Laws and considering the suggestions received, the Council recommended that the six vacancies for ordinary members be filled by the election of Professor G. A. Barnard, Dr. W. R. Buckland, Miss M. Deane, Mr. R. F. George, Mr. W. B. Reddaway and Mr. C. T. Saunders. No alternative proposals were received and the Fellows named below will be announced at the Annual General Meeting on June 18th, 1958, as having been elected President, other officers and other members of Council for the Session 1958–59. (Those whose names are marked * were not members of Council during the previous Session; those marked † have not previously served on the Council.)

President

Sir Harry Campion, C.B., C.B.E.

Council

N. T. J. Bailey.
*G. A. Barnard.
D. G. Beech.
*W. R. Buckland.
D. R. Cox.
M. P. Curwen.
H. E. Daniels.
†Marjorie Deane.
J. Durbin.
E. C. Fieller.
R. F. George.
D. Heron.
W. N. Jessop.
J. O. Irwin.
M. G. Kendall.
F. A. A. Menzler.
L. Moss.
†W. B. Reddaway.
D. D. Reid.
†C. T. Saunders.
W. F. Searle.
Sir Robert Shone.
C. A. B. Smith.
A. Stuart.
H. Tetley.
L. H. C. Tippett.

Honorary Treasurer

R. F. Fowler.

Honorary Secretaries

B. Benjamin Iris Douglas P. Armitage

On behalf of the Council,

H. Campion, *President.*

R. F. George
Iris Douglas
B. Benjamin
} *Honorary Secretaries.*

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APPENDIX A

From June, 1957 to May, 1958 inclusive, the candidates named below were elected Fellows of the Society:—

Addison, Gordon.
Adeyinka, Sunmola Olatunji.
Armstrong, Robert Rushby.
Ashby, Roger Harold.
Assee, Lloyd Earl.

Baillod, Paul André.
Bakes, Michael David.
Banks, John Neville.
Barratt Brown, Michael.
Baum, Myra.
Beattie, John Moffat.
Beech, Thomas Hugh.
Beesley, Eric William.
Begg, Alan Duncan.
Benjamin, Sidney.
Bennellick, Peter John.
Bennett, John Makepeace.
Berry, Brian Joe Lobley.
Best, Richard Darcy.
Board, Douglas Basil Mullins.
Bodmer, Walter Fred.
Bramley, Harry.
Budge, Victor Lawrence.
Burton, John Donne.
Butler, Donald.

Campbell, Isabella Nimmo.
Cannell, Thomas Walter.
Chalk, Dorothy Selina.
Chang, Tse Fo.
Chatterley, Francis John.
Chellew, Tom.
Claringbold, Peter John.
Clark, Kenneth Edward.
Collett, George Walter.
Coore, Haldane Gladstone.
Cownie, Eric Hugh.
Cox, John Burnett.
Cramer, Jan Salomon.
Croizat, Robert Marcel.
Crossley, John Rodney.

Davies, Alun.
Davies, Frederick Byron.
Davis, John Roberts.
Davis, Norman Henry William.
Dhital, Vishnu Prasad.
Dobbs, Alan Gordon Richard.
Doherty, William Eugene.
Douglas, James Bertram.
Duncan, David Conochie.
Dyson, Joyce.

Elliott, Ronald William.
Elmslie, James Reginald Carrington.
van Embden, Josua.

Feeney, George Joseph.
Feldman, Stephen Trevor.
Fessey, Mereth Cecil.
Fison, Bernard Hayden.
Fitzpatrick, Rupert.
Flowerdew, Anthony David John.
Forbes, Jean.
Fowler, Marjorie.
Francois, Peter Eugene.
Fraser, George Robert.
Fry, James Frederick.

Galambos, Paul.
Garland, Ivor.
Gibbons, Margaret Elizabeth.
Goff, Merritt Milford.
Goldstone, Leo.
Grant, Joseph William.
Grant, Robert Wasson.
Green, Ronald William.
Gribbin, Thomas Kevin.
Gupta, Ram Krishna.
Gupta, Shugan Chand.

Hards, Alan Ernest Charles.
Harman, Colin Sidney.
Harris, Henry Roy.
Harrison, Peter Jeffrey.
Hart, Ralph Thomas.
Hartshorn, Jack Ernest.
Henley, John Maybin.
Henstock, Ralph.
Hicks, Terence Edwin.
Hill, Thomas Henry.
Hills, Mary Georgina.
Holt, James Thomas Dennis.
Howarth, Alfred.
Hudd, George Herbert.
Hunter, David Boss.

Ibrahim, Fathi Mohamed.
Ingram, Gilbert John.
Ironmonger, Duncan Standon.
Izzard, Charles William.

Jain, Jia Lal.
James, William Lloyd.
Jarvis, Julian Louis.
Jeffreys, John Lewis.
Johnson, André Michel.
Jones, Roy Andrew.

Kahan, Basil Charles.
Katz, Leo.
Kelly, Dermott Frederic d'Arenberg.
Kendell, Peter James.
Kernan, Dennis.
Khin Maung Lwin.
Kikuchi, Susumu.
King, Sidney.
Kirby, David John.
Knox, Ernest George.
Knox, John Rigby.

Lavalette, Eric Eustace Canram.
Lawrie, Norman Lamont.
Lewis, Peter.
Littler, Keith Trevor.
Livingstone, James McCardle.
Lloyd, Ian Stewart.
Lloyd, Judith Mary.
Longman, Brian Owen.
Loveland, Christine Margaret.
Loveluck, Clive.

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McCarthy, Michael Donald.
Maciver, Murdo John.
Maher, Peter.
Majr, Charles.
Mann, Paul Henri.
Mansfield, Louis Cunningham Swift.
Martin, William Ian Robertson.
Mathieu, Jean Claude Gerard.
Medin, Knut Hakan.
Melinek, Ivan.
Mendenhall, William.
Middleton, George Davidson.
Morling, Albert Walter.
Morris, Bryan Ralph.
Morris, George William.
Morton, Robert George.
Munro, Ross Donald.
Mursi, Abdel Moneim.
Murphy, Timothy.
Murthy, Vrudhula Krishna.

Nankivell, Owen.
Newell, Frederic William.
Nixon, Eric.
Nyunt, K. M.

O'Higgins, Charles Louis.
Oldfield, William.
Overton, Gloria May.

Pace, Maurice.
Pappas, Peter Demetrius.
Paris, John Woodrow Stanislaus.
Pearlman, Vera.
Pedoe, Joseph.
Pender, Robert William.
Penny, Angela Hazel Alberta.
Peters, George Henry.
Potter, Jack.
Price, Eric Hardiman Mockford.
Price, Hugh Griffith.
Prior, William Edward.
Proctor, James Nelson.
Proctor, Jean Rosemary.
Puri, Arjun.

Ractliffe, John Fuller.
Ramasubban, Turayur Ananthan.
Ramshandani, Gulab Nanikram.
Ramsden, Geoffrey.
Randhawa, Harcharan Singh.
Reiersøl, Olav.
Reinger, Edwin Jacques.
Rhodes, Geoffrey James Eveleigh.
Rigby, Lucy.
Roberts, Celia Mary.
Robinson, George Harvey.
Roots, John Christopher Herbert.
Rose, Sally Elizabeth Acland.
Roth, Raymond E.
Rothman, Maurice.
Rudd, Riley Anthony Winton.

Saffell, Derek Jarvis.
Sayer, Brian William.
Shaw, Donald Curtis.
Shaw, Leonard Walter.
Shepherd, Noel Spencer.
Shepherd, Wilfred Alec.
Sirker, Aman Krishna.
Soedarjono, Raden.
Smart, Kenneth Anderson.
Smithson, John Frederick.
Spackman, Deryck Reginald.
Sterminski, Zygmunt Antoni.
Stimpson, Brian Curtis.
Stocks, John Macdonald Bennett.
Stonham, Paul Edward.

Tapp, Patricia Ann.
Taraporevala, Russi Jal.
Tassini, Raymond James.
Tate, Elizabeth Anne Markham.
Thomas, David Anthony Hayden.
Thomas, John James.
Thomas, Sonja.
Trollope, Robert Basil.
Tuffaha, Shawkat Ahmad.

Ugalde, Gregory.

Valinsky, David.

Wagstaffe, Norman Thomas.
Wainwright, Sam.
Walker, Dennis.
Ware, John James Blake.
Watterson, Geoffrey Anton.
Weir, Joyce Margaret.
Welch, David Theodore.
Wheller, Barbara Elizabeth Anne.
White, Louis Stuart.
Whyte, Eric John.
Williams, Leonard.
Winterford, Charles William.
Wong, Vivian Leopold Alexander.

Yau, Richard Choi Kwong.
Young, Herbert Timothy.

Corporate Representatives

Bailey, David Earle, *representing* A. V. Roe and Co. Ltd., Weapons Research Division.
Barrow, Arthur James, *representing* Hogg and Mitchell Ltd.
Green, Peter Gorton, *representing* Gillette Industries Ltd.
Jackson, Anne Katherine, *representing* the National Institute of Economic and Social Research.

Jones, Grace Mary, *representing* the Ministry of Pensions and National Service.
Kühnberg, Walter, *representing* the Financial Times Ltd.
Land, Ailsa Horton, *representing* the Research Techniques Unit, London School of Economics.

Morojele, C. M. H., *representing* the Department of Agriculture, Basutoland.
O'Connor, Laurence Kevin, *representing* the Milk Marketing Board.
Kent, John Harold Stanley, *representing* the Hulton Press Ltd.

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STATEMENT OF INCOME AND EXPENDITURE

1956		EXPENDITURE	1957	
£	£		£	£
	109	Rents payable *less* rent received		130
	491	House expenses		794
	3,310	Salaries and wages (including contribution to staff superannuation scheme)		3,683
	250	Pension		250
	14	Insurance		42
	55	Office and Library equipment		89
	173	Postage and telephone		201
	602	Stationery and miscellaneous printing		558
		Publication and distribution expenses:		
4,579		Journal, Series A (General) and reprints	5,056	
4,770		Journal, Series B (Methodological) and reprints	1,703	
	9,349			6,759
	15	*Applied Statistics, loss for year*		—
		Library:		
390		Books	335	
330		Binding	104	
	720			439
	182	General Meetings—ordinary and annual		216
	51	Council and committee travelling expenses		20
		Expenses of Sections:		
26		Research	49	
243		Industrial Applications	300	
48		Study	63	
7		Medical	6	
	324			418
	—	Guy Medal		5
		Examination expenses:		
74		Printing, stationery, postage, etc.	70	
98		Examiners' fees	101	
	172			171
	53	Auditor's fee (1956)		53
	67	Miscellaneous expenses		102
	425	Amortisation of leasehold premises		425
	—	Income tax irrecoverable		20
	16,362			14,375
	—	Surplus carried to Accumulated Fund		1,920
	16,362			16,295
	233	Amount carried to Life Composition Fee Fund		332
	£16,595			£16,627

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DIX B

FOR THE YEAR ENDED 31ST DECEMBER, 1957

1956 £	£	INCOME	1957 £	£
		Annual subscriptions:		
6,856		Fellowship	7,299	
10		Research Section	13	
87		Industrial Applications Section	92	
11		Study Section...	11	
6		Medical Section	9	
——	*6,970*		——	7,424
	50	Contribution from Royal Economic Society		50
	1,264	Dividends and interest (gross)		1,342
		Sales of Journal:		
3,371		Journal, Series A (General) and reprints	4,077	
3,115		Journal, Series B (Methodological) and reprints	2,829	
——	*6,486*		——	6,906
	—	*Applied Statistics*—Surplus for year		271
	25	Sales of other publications		22
	237	Examination receipts: fees and sale of papers		192
	95	Surplus from Conferences		49
	110	Transfer from Life Composition Fee Fund, being fees of compounders known to have died during year and of those presumed to have died in previous years		39
	20	*Profit on sale of investment*		—
	15,257			16,295
	1,105	*Deficit carried to Accumulated Fund*		—
	16,362			16,295
	233	Life Composition Fees received during year		332
	£16,595			£16,627

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BALANCE SHEET AT

FUNDS AND LIABILITIES

1956 £	£		1957 £	£
		Accumulated Fund:		
10,158		Balance at 31st December, 1956	11,653	
2,600		*Add:* Bequest by Mr. Udny Yule	136	
12,758			11,789	
—		*Less:* Loss on Sale of Investments 720		
—		Past service contribution to staff superannuation scheme 1,911		
			2,631	
12,758			9,158	
—		*Add:* Balance per Statement of Income and Expenditure ...	1,920	
1,105		*Less: Balance per Statement of Income and Expenditure* ...	—	
	11,653	Balance (subject to unrealised depreciation of investments *per contra*)		11,078
		Life Composition Fee Fund:		
7,799		Balance at 31st December, 1956	7,922	
233		*Add:* Life Composition Fees received during year	332	
8,032			8,254	
		Less: Transfer to Statement of Income and Expenditure of fees of compounders known to have died during year, and of those presumed to have died in previous years	39	
110				
	7,922			8,215
		Building Fund:		
	1,661	Balance at 31st December, 1956		1,661
		1950 Benefaction Fund:		
	30,971	Balance at 31st December, 1956		30,971
		Frances Wood Memorial Fund:		
594		Balance at 31st December, 1956	617	
23		Income for year	30	
	617			647
		Liabilities and Income held in suspense:		
5,061		Sundry creditors	3,359	
1,084		Amounts received in advance	3,196	
	6,145			6,555
	£58,969			£59,127

REPORT OF

I have examined the above balance sheet and annexed statement of income and expenditure December, 1957, and of its income and expenditure for the year ended on that date according to

5, London Wall Buildings,
London, E.C.2.
19th, May, 1958

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DIX C

31ST DECEMBER, 1957

ASSETS

1956 £	£		1957 £	£
		Leasehold Premises:		
21,368		At Cost	21,368	
2,046		*Less:* Amortisation to 31st December, 1957	2,553	
19,322				18,815
		Investment of Leasehold Sinking Fund:		
	2,046	At cost, per schedule attached		2,553
		(Market Value 31/12/56, £1,802; 31/12/57, £2,190)		
		General Investments (including investment of Frances Wood Memorial Fund):		
	29,067	At cost, per schedule attached		30,747
		(Market Value 31/12/56, £25,772; 31/12/57, £24,896)		
		Current Assets:		
1,539		Debtors and payments in advance	1,492	
100		Arrears of subscriptions recoverable (estimated)	100	
20		Income tax recoverable	196	
6,875		Balances at banks and cash in hand	5,224	
8,534				7,012
£58,969				£59,127

NOTE.—
No value is placed in the Accounts on—
(a) Journals and other publications in stock.
(b) Books in library.
(c) Pictures, furniture and equipment.

H. CAMPION, *President.*

R. F. FOWLER, *Honorary Treasurer.*

THE AUDITOR
which, in my opinion, respectively give a true and fair view of the Society's affairs as at 31st the best of my information and the explanations given to me and as shown by the books.

W. G. DENSEM,
Chartered Accountant.
(Deloitte, Plender, Griffiths & Co.)

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APPENDIX D

SCHEDULE OF INVESTMENTS—31ST DECEMBER, 1957

1956			1957		
Market Value	*Cost*		*Nominal*	*Cost*	*Market Value*
£	£			£	£
		General Investments:			
6,701	*6,765*	2½% Consols	—	—	—
2,573	*2,985*	3½% Conversion Loan	£130	101	82
351	*490*	3½% War Loan	—	—	—
2,179	*2,992*	3% Savings Bonds 1965–75	£2,970	2,992	2,094
484	*400*	British Transport 3% Stock 1978–88	—	—	—
11,540	*13,415*	British Electricity 3½% Stock 1976–79	£9,526	8,672	6,859
1,944	*2,020*	British Electricity 4¼% Stock 1974–79	£2,246	2,020	1,864
—	—	Newcastle-upon-Tyne Corporation 6% Redeemable Stock 1973-76 (10% paid)	£100	10	12
—	—	Bowater Paper Corporation Ltd. 5¾% Conv. Unsecured Loan Stock 1978–82	£1,000	1,059	890
—	—	British Petroleum Co. Ltd. 5% First Debenture Stock 1974-78	£1,000	939	835
—	—	6% Conv. Debenture Stock 1976-80 (20% paid)	£50	10	12
—	—	Metal Box Co. Ltd. 5½% Unsecured Loan Stock 1977-80	£1,000	980	925
—	—	Peninsular & Oriental Steam Navigation Co. Ltd. 5% Debenture Stock 1975-80	£1,000	975	845
—	—	Tube Investments Ltd. 5¼% Unsecured Loan Stock 1977-82	£1,000	955	890
—	—	Ferranti Ltd. 7% Cumulative Preference Stock	£850	1,012	903
—	—	Associated Portland Cement Manufacturers Ltd. Ordinary Stock	£430	989	712
—	—	British Insulated Callender's Cables Ltd. Ordinary Stock	£370	977	745
—	—	English Electric Co. Ltd. Ordinary Stock	£330	957	784
—	—	Foreign, American & General Investments Trust Ltd. Deferred Stock	£450	1,006	872
—	—	Globe Telegraph & Trust Co. Ltd. Ordinary Stock	£450	984	878
—	—	Guest, Keen & Nettlefolds Ltd. Ordinary Stock	£520	1,286	1,222
—	—	Imperial Chemical Industries Ltd. Ordinary Stock	£430	978	822
—	—	International Combustion (Holdings) Ltd. Ordinary Stock	£175	948	735
—	—	Plessey Co. Ltd. Ordinary 10*s.* Shares	250	985	750
—	—	Pye Ltd. "A" Deferred Ordinary Stock	£250	922	625
—	—	Albert E. Reed & Co. Ltd. "A" Ordinary £1 Shares	400	990	540
£25,772	*£29,067*			£30,747	£24,896
£1,802	*£2,046*	*Investment of Leasehold Sinking Fund:* British Electricity 3½% Stock 1976–79	£3,042	£2,553	£2,190

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PROCEEDINGS OF THE ONE HUNDRED AND TWENTY-FOURTH ANNUAL GENERAL MEETING OF THE ROYAL STATISTICAL SOCIETY held at the London School of Hygiene and Tropical Medicine on Wednesday, June 18th, 1958, at 5.30 p.m.

The Chair was taken by the President, Sir Harry Campion, C.B., C.B.E.

The President presented the Report of the Council for the financial year 1957 and the session 1957–58, and moved that it be received.

During his submission of the Report the President presented the Guy Medal in Silver to Professor G. A. Barnard, and Certificates to the successful candidates in the 1958 examination who were able to be present at the meeting.

The Honorary Treasurer, Mr. R. F. Fowler, presented the accounts and seconded the motion that the Report be received.

On being put to the meeting the Report was formally received.

On the proposal of Mr. F. A. A. Menzler, seconded by Mr. W. F. Searle, Mr. W. G. Densem was unanimously re-elected Auditor of the Society for the session 1958–59, at a fee of fifty guineas.

The President announced that as no alternative nominations had been received the President, Council and Officers for the session 1958–59, nominated as shown on the list already circulated, were duly elected.

The meeting then terminated.

The Annual General Meeting was preceded by an Ordinary Meeting at which the following statisticians were elected Honorary Fellows of the Society:

Professor Simon Kuznets	United States.
Professor Jerzy Neyman	United States.
Professor J. Tinbergen	Netherlands.

The candidates named below were elected Fellows of the Society:

Joseph Alfred Frobisher Alltoft
Leonard William Alltoft
Peter Chandler
Vivienne Florence Laura Cherriman
Brian Wallace Conolly
Rosemary Susan Hayes
Ernest Harry Haynes
Namet Ilahi
Alfred Geoffrey Rimmer
Colin Vinten Ulyatt
Brian Edward Wilson

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REVIEWS OF STATISTICAL AND ECONOMIC BOOKS

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1.—*A Course in Multivariate Analysis.* By M. G. Kendall. (Griffin's Statistical Monographs No. 2.) London, Griffin, 1957. 185 pp. 8½″. 22*s*.

The field of multivariate analysis (in the wide sense considered by Professor Kendall) has perhaps been subject to a more erratic and unequal development than any other in statistics. This is partly due to its early origins which have permitted some areas, notably Factor Analysis and some of the theory of Functional Relationship, to evolve apart from the main body of statistical theory and to become prematurely ossified in the elaborate and often insecurely based structures of psychology and economics whilst others have become embedded in classical, non-statistical, least squares theory. Partly, too, it is due to the native complexity of the subject which has not only necessitated a heavy algebraic development, with consequently laborious computational procedures, but also, more critically, has imposed an invidious choice from a number and multiplicity of possible theoretical models and askable questions (of estimation and hypothesis-testing) manifoldly exceeding those with which we are accustomed to deal in the "univariate" theory. These two aspects have interacted; the heavy computations discouraging the use and verification of the theory and the wide choice of models and questions tying the theoretical developments to special applications (and associated jargons). And, whilst it is both right and proper for applied statistics to be firmly rooted in their field of application, there comes a time when the theory needs to be gathered from the journals of psychology, economics, mathematical statistics, etc., and sorted out, presented and developed in its own right so that it may be applied in any field where the requisite conditions hold. This time has long since passed in the case of multivariate analysis and although there has been a number of review-papers and purely theoretical developments and some very clear statement of parts of it in text-books such as Rao's, an outstanding need for a book-length survey has long been felt.

Statisticians must count themselves fortunate that one so skilled in exposition and of so encyclopaedic a grasp as Professor Kendall has taken on the task of guiding them through the techniques of Canonical Analysis, Component Analysis (and its less respectable *alter ego*, Factor Analysis) through the problems of Functional Relationship (with its attendant mysteries of Unidentifiable Parameters and Inconsistent Maximum Likelihood Estimation) and through the elegant and now almost classical Multivariate and Discriminant Analysis associated with the names of Wishart, Wilks, Hotelling, Karl Pearson, Fisher and Maha-

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lanobis (together with its more recent developments). The sometimes extremely subtle differences between models are carefully dissected with full numerical illustration, and many obscurities are clarified. But it must not be thought that the statistician's path is now clear, for Professor Kendall's survey has thrown the considerable gaps in theory into sharp relief and we may hope that perhaps the most important contribution of this book will be to stimulate an enlightened and invigorated assault on the outstanding problems. This is underlined by the fact that although the book consists basically of lectures given in 1954, and privately circulated then, the revision needed to bring it up to date has consisted only of the addition of a few titles to the list of references. (The one exception to this remark, in fact, shows this stimulant already at work.) A second result to be hoped for is the discarding or supercession of many of the facets treated here which will surely be regarded as abortive in the light of future work. This will be facilitated by the great increase in range and quantity of practical applications that will equally surely be stimulated, particularly in view of the recent increase in accessibility of electronic computers for statistical purposes.

The book, then, may be regarded as an interim statement of the state of the theory, thus excusing the absence of an index and the oddly eclectic selection from the references in the text given in the end-list, and accounting for the fact that Professor Kendall has had to concentrate relatively more attention on the more obscure and confused parts of the theory than their ultimate importance may warrant. The reader will be well advised to follow up the list of further reading recommended by Professor Kendall and perhaps to embark on a preliminary revision course of Chapter 28 of his *Advanced Statistics*, Vol. II (Kendall, 1951), or the latter chapters of Wilks (1943) (both for some reason omitted from the end list of references) and also Rao (1952, Chs. 2, 7–9).

References

KENDALL, M. G. (1951), *The Advanced Theory of Statistics*, Vol. 2. 3rd ed. London: Griffin.
RAO, C. R. (1952), *Advanced Statistical Methods in Biometric Research*. New York: Wiley. London: Chapman & Hall.
WILKS, S. S. (1943), *Mathematical Statistics*. Princeton, N.J. London: O.U.P.

D. E. BARTON.

2.—*Some Aspects of Multivariate Analysis*. By S. N. Roy. New York, Wiley, 1957. Calcutta, Indian Statistical Institute (London, Chapman & Hall). [ii]. viii, 214 pp. 11″. 64*s*.

This book is almost certainly the most advanced, if not the most highly specialized, full-sized work on multivariate analysis now in existence. It consolidates a series of researches which have been undertaken by its distinguished author in recent years: for this reason it is not a book to be used by those seeking an authoritative and balanced general treatment of the subject. This does not, of course, imply that Professor Roy's work is incorrect or unbalanced, but it does reflect, to a remarkable degree, the particular interests, and methods of approach, of its author.

Professor Roy puts forward two original suggestions as philosophical bases for the development of his system of statistical analysis. In the construction of tests—say when testing a simple hypothesis H_0 against a set Ω of simple alternative hypotheses H— he proposes to use critical regions formed by the union of regions $\omega(H, H_0)$ defined by the inequalities

$$p(x|H) \geqslant \lambda(H, H_0)\, p(x|H_0)$$

with either

(i) $\lambda(H, H_0)$ varying in such a way that the tests of H_0 with critical regions $\omega(H, H_0)$ each have the same significance level β—(tests of "Type I"), or

(ii) $\lambda(H, H_0)$ constant—(tests of "Type II").

The author shows that these two types of test are each unbiased in the general case: in a number of special cases he has succeeded in obtaining the power of the test and shows that it is quite high. For many of the cases considered in the present volume, however, the

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exact power is not available, nor are close numerical bounds for the power. This detracts somewhat from the force of the author's argument, although it is shown, fairly convincingly, that the tests should have reasonably good properties as compared with other tests already suggested.

In multivariate analysis, problems of estimation often call for the calculation of confidence intervals for a large number of parameters, e.g. for the population mean values of a number of different variates. The author suggests that, such "simultaneous confidence intervals" should be obtained subject to the condition that the probability that *none* of them is incorrect should not be less than a preassigned (generalized) confidence coefficient. Ingenious derivations of appropriate methods of obtaining such sets of "simultaneous confidence intervals" are provided but there are no numerical examples. Experience in simpler cases leads the reviewer to suspect that Professor Roy's requirements are likely to lead to inconveniently wide confidence intervals in many practical applications.

The book includes also a chapter on multiple contingency tables, which the author regards as a non-parametric generalization of multivariate analysis of variance. The substance of this chapter has appeared in a paper in a recent issue of *Biometrika*.

There are nine appendices giving much needed mathematical derivations of results used in the text. The amount of mathematical argument needed can be judged from the fact that these appendices occupy no less than eighty pages—considerably more than one-third of the book.

There are a number of minor misprints, particularly in suffices, superfices etc. in the complicated formulae which appear on most pages of the book.

This should prove an unusual and stimulating book for those with the time, ability and inclination to master its contents. It should certainly be read by anyone engaged in research, whether in multivariate analysis, or in many other topics in statistical theory. It would be improved by more attention to numerical aspects of the problems considered. In particular, tables of the distribution of the largest and smallest "canonical correlations" (characteristic roots of a certain matrix) would be useful, since a substantial part of the theory is centred on these statistics.

N. L. JOHNSON.

3.—*Introduction to Multivariate Statistical Analysis*. By T. W. Anderson. New York, John Wiley (London, Chapman & Hall, 1958). xii, 374 pp. 100*s*.

Multivariate analysis has been, for some time, a field of research with special attraction for the mathematician-turned-statistician with a taste for complicated algebraic manipulations and applications of calculus. The services of these specialists have led to rapid development of concepts and "theorems", not always, unfortunately, accompanied by any serious attempt to decide whether the new techniques would be applicable to real situations and data, outside the lecture room. Nor, until recent years, have there been any really consistent efforts to codify the knowledge so gained into a more or less coherent whole. This latter shortcoming is now, however, rapidly being removed, though there still seems to be little more than lip-service for the practical application of the methods developed.

The present text-book is a solid and competent attempt at systematizing the subject, with special reference to the needs of undergraduate or immediate post-graduate students. Although applications still occupy a definitely minor position, the impression is gained that the author really has done his best, in present circumstances, at any rate, to show the way to apply as many of the techniques as he can.

Matrices are used throughout the book, and the bold type used lends a falsely formidable appearance to the text. But provided the intending reader is not intimidated by cumbersome appearances, he will find he is reading a text of considerable lucidity and power.

The first hundred pages of the book are devoted to introduction of notation and establishment of standard statistical results, with particular reference to multiple linear regression, in terms of this notation. Multivariate analysis, as generally understood, starts with the fifth chapter with a discussion of the generalized (Hotelling's) T^2 statistic. The

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sixth chapter contains an adequate and useful discussion of the classification problem (often termed discriminatory analysis). Many workers find this the most useful section of multivariate analysis, though the theoretical aspects are relatively simple. Chapter 7 develops the Wishart distribution and associated distributions, and the following chapter applies the general linear hypothesis, and analysis of variance, to multivariate analysis. Chapters 9 and 10 discuss various likelihood ratio tests, in particular tests for equality of variance-covariance matrices and for equality of expected value vectors, and for independence of sets of variates.

The last four chapters contain a mixture of assorted techniques, among them short chapters on Principal Components and Canonical Correlations.

There is a useful summary of necessary matrix theory in an appendix covering some twenty pages. A further appendix gives an extensive, though not exhaustive, bibliography.

There is an ample supply of problems at the end of all but the first and last chapters; unfortunately solutions are not given, though the student should find very useful the references to the sections of the preceding chapter particularly applicable to each problem.

This book does provide a really effective student's text-book in what is practically a fresh field for such books, but it is a field which will probably still remain a highly specialized one for many years. This may account for the exceptionally high price, which is justified by the quality of the book, but which may well have an unfortunate effect on its sales.

N. L. JOHNSON.

4.—*Readings in Linear Programming*. By S. Vajda. London, Pitman, 1958. vii, 99 pp. 8½". 20*s*.

Linear programming and the related subjects of activity analysis and the game theory have developed rapidly in recent years as a result of the stimulus arising from the interest and usefulness of these subjects to research workers engaged in the study of economic, military, industrial and mathematical problems. Linear programming computations consist of simple arithmetic processes, although the mathematical proofs and problems of formulation are sometimes quite deep and technical. This little collection of (partly) worked examples in the arithmetic of linear programming is primarily aimed at readers whose mathematical knowledge is slight but who wish to acquire a knowledge of and familiarity with some of the simpler methods of linear programming from the operational research standpoint.

The treatment is self-contained and avoids all but the most elementary algebra. It would probably be desirable, however, for a beginner to study an elementary introduction to the *theory* (such as that written by Vajda himself). The method used is to lead the reader systematically through a very concise treatment of a series of simplified practical problems which have been treated more fully in the journals and more advanced texts.

These examples should prove useful in an introductory course on operational research for non-mathematical management personnel, or in an elementary course on "mathematics for economists".

Alternative approaches to the transportation (distribution) problem—with and without degeneracy—in its various manifestations (catering, productions scheduling, bid evaluation, etc.) are discussed, together with some closely related problems, in the first forty-five pages. The next forty-five deal with applications of the "simplex method" and of the "regrouping (duality) principle" in problems of a more general type.

The use of perturbation methods is not treated ; the author justifies this by pointing out that although the simplex process given *may* cycle indefinitely without ever terminating if the optimal solution is degenerate, yet this has never, in fact, occurred in practice, so that the risk of such an occurrence may be regarded as negligible.

The last chapter deals with the use of linear programming methods in game theory and thus helps to show the close mathematical relationship between these two subjects.

Some minor criticisms are as follows : (i) The presence in the text of a number of slips

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or misprints, none of which should cause any difficulty. (ii) The economic significance of the duality principle and of the "imputed (shadow) valuations" is not stressed, although use is made of it in dealing with transportation programming, and also on p. 76. (iii) The formulation of a linear programme for problems involving uncertainty is not discussed, apart from the chapter on "games". In consequence, neither the computational problems, nor the introduction of a "risk-aversion" element into the objective function, which arise when the data are in probabilistic form, are treated.

In short, the book is a useful short elementary introduction to the arithmetical treatment of a wide variety of practical problems, with guidance for more advanced and extensive reading, and a useful bibliography. J. WISE.

5.—*Programme linéaire—Agrégation et Nombres Indices.* (*Cahiers du Séminaire d'Econométrie*, No. 4. Ed. R. Roy.) Paris, Centre National de la Recherche Scientifique, 1956. 146 pp. 9½". fr. 750.

This fourth collection of econometric essays follows naturally from the third and is principally concerned with the problem of aggregation, a field in which French econometricians have been particularly active.

A. Nataf contributes a survey of the development of the study of aggregation since Klein posed the question (in *Econometrica*, 1946) of the relationship between macroeconomic functions and the established microeconomic theory of the behaviour of persons and firms whose individual decisions underlie the aggregate function.

"Linear aggregation of systems of linear equations" by M. H. Theil presents the most comprehensive study to date of the rigorous theoretical desiderata for valid aggregation. In "Aggregation in economic models", E. Malinvaud develops the thesis that the form of aggregation depends, with the rest of the model, upon the object of the research. He draws upon Wald's decision theory to develop a minimax principle to determine the degree of aggregation of a Leontieff model.

"Some critical observations on index numbers" by A. Winkler relates naturally to the topic of aggregation, but the inclusion of M. Ragnar Frisch's paper, "The solution of linear programming problems by the logarithmic potential method", seems rather arbitrary in view of the homogeneity of the rest of the volume. This method is, of course, available in greater detail (in English) in the form of memoranda of the University Institute of Economics, Oslo. One would therefore have preferred to have seen a wider ranging discussion of Professor Frisch's experience of using linear programming on macroeconomic problems. It seems a pity that the logarithmic potential method and the simplex method for solving linear programmes should be presented as competitors, when it seems highly probable that a compromise of striking through the feasible region to a point near the optimum by Frisch's method, followed by a few simplex iterations to the true solution, would be a very powerful procedure.

In the discussion following his paper, Professor Frisch states that the "hours of work" for the simplex method rise by the cube of n (number of degrees of freedom, or excess of the number of variables over the number of equations) as compared to the logarithmic potential method where the amount of work rises by the square of n. This is a somewhat unfair comparison since in the inverse basis version of simplex n enters only as a linear term into the formula for the amount of work, although m, the number of equations, enters as a cubic term. This seems to suggest that simplex has the advantage in problems where the number of variables greatly exceeds the number of constraints. This is the typical case in industrial problems, though apparently not in the type of national study with which Professor Frisch has been most actively concerned.

A. H. LAND.

6.—*A First Course in Statistics.* By Robert Loveday. Cambridge, University Press, 1958. ix, 121 pp. 8½". 8*s.* 6*d.*

The teaching of elementary statistics as a school subject has for too long been handicapped by the lack of text-books comparable in size, scope and price with the text-books used in

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other branches of school mathematics. The appearance of a text-book designed to meet the needs of Ordinary Level G.C.E. students, with the promise of a second book extending the subject to Advanced and Scholarship Levels, is therefore to be welcomed. The harassed school teacher of mathematics is now provided with a ready-made coherent course and his students have at hand a source of carefully selected exercises.

The author's approach is simple and direct. His purpose is to introduce the student to the basic tools of statistical analysis—to averages, measures of dispersion, regression and correlation—with the minimum of fuss and verbiage. In attempting this difficult task the author's long experience of teaching elementary statistics to young students has enabled him to select his material with discrimination and to arrange it in a sequence acceptable to beginners. Though his style is terse and therefore necessarily somewhat dogmatic, at least it is always clear and unambiguous. Though the explanations are sometimes incomplete and some formulae are necessarily given without proof, at least these deficiencies will provoke from some students the questions that will lead them to the next stage of statistical analysis. The publication of this carefully prepared slim volume should do much to widen the general appreciation of statistical methods of numerical analysis.

Some doubts arise, however. Perhaps the author has succeeded too well in his task. Users of this text-book may pass examinations but the subject is presented as a neat and tidy collection of techniques to be learnt and applied forthwith to data suitably pre-organized for analysis. The student reading this text-book may come to regard statistics as a subject in which the only question that arises (and that only when the data are suitably arranged for him) is : Which of the techniques do I apply here? He will be encouraged to accept without question both the data put before him and the validity of the questions asked. If the teacher who adopts this book wishes to avoid this danger he will need to supplement the exercises offered here by giving his students some practice in collecting, arranging, analysing and interpreting data that have some relevance to the pupil's own experience and interests.

B. C. BROOKES.

7.—*Numerical Methods.* By R. A. Buckingham. London, Pitman, 1957. xii, 579 pp. $8\frac{1}{2}''$. 70*s*.

In view of the considerable number of books which have appeared in recent years dealing with numerical analysis, it might well be asked whether an additional general text-book can be justified at this stage. There is no question, however, that this is an excellent book and it is very likely to become an accepted text for university teaching—in the reviewer's opinion, it is the most comprehensive work available at the moment.

Many books on numerical methods either have a tendency to concentrate on special topics of interest to the authors, or else are recipe books giving a variety of methods supported by examples which at best illustrate the relevant computing schemes. This book suffers from neither of these defects—it deals with all topics of importance except integral equations (there are short sections only dealing with the Volterra and Fredholm types) and careful attention is paid to the choice of methods or combination of methods which is best suited to a particular application.

Special features of the book are the working of about 150 numerical examples, admirably set out with checks and useful comments, the detailed reference to work published in the last ten years and the widespread use of matrix methods which are given almost as much prominence as finite difference methods. The book is written in the main for the competent mathematician (the theoretical examples at the end of each chapter bear witness to this), but a very useful feature is the self-contained nature of the chapters. Thus, the basic ideas in matrix algebra, differential equations, the theory of iterative processes, etc., are described adequately so that there is no necessity to refer to a more theoretical text.

After a useful introductory chapter dealing with the basic notions of computing, there are five chapters devoted to finite difference methods applied to interpolation, numerical integration and differentiation. The last of these is an extremely good concise chapter on symbolic methods which draws heavily on Prof. W. G. Bickley's work on this topic. Much of the remainder is standard but it is treated very critically here and the reader who is

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prepared to study the worked examples in detail will find very useful hints on such things as choice of formula and the effect of interval length on round-off and truncation errors ; also of interest is the unusually large amount of space given to summation formulae and factorials.

Next follow two chapters on differential equations which are characterized by the excellence of the examples in which the basic methods are supplemented by others such as Taylor series (for small x) and asymptotic series (for small $y = x^{-1}$). These are followed by excellent chapters on algebraic equations, matrix methods applied to simultaneous linear equations, and latent root problems for matrices, the latter being one of the best in the book. There are also later chapters dealing with classical iterative methods and relaxation applied to ordinary linear equations and to differential equations with two point boundary conditions, difference techniques applied to functions of two variables and a more than useful introductory chapter on partial differential equations.

There is also a chapter on least squares theory but as in all books on numerical analysis, the treatment is unsatisfactory from a statistical point of view. It is suggested that this chapter could profitably be rewritten in future editions of this book.

The method of least squares (attributed to Legendre with no mention of Gauss) is said to be linked to the normality of the error distribution ; to set the theory in perspective, it is necessary to quote Aitken's generalization of the Markoff Theorem dealing with correlated residuals and unequal variances. Numerous methods are described for dealing with equally spaced data but the comparison of methods on page 323 is beside the point from statistical considerations where the orthogonal polynomials play a fundamental role. One would also have liked to see discussed the question of orthogonal polynomials for unequally spaced data (see Hayes, J. G. and Vickers, T., *Phil. Mag.* (1951) and Forsythe, G. E., *J. Ass. Comp. Mach.* (1958).

As an example of non-linear least squares equations, Prony's method of fitting a sum of exponentials is described. This effectively reduces the problem to one of fitting a difference equation involving the original observations which is identically satisfied if there are no errors. Although not mentioned in the text, the appeal of the method is that it retains information about damping factors and frequencies whilst eliminating that concerned with phase and amplitude which are initial conditions anyway and of secondary interest. These latter quantities are then obtained by another least squares fitting assuming the frequencies and damping factors known. This method of approach may be satisfactory when the data are almost exactly fitted by the assumed relationship (as in the example given) but in general this will not be the case and the question of the efficiency of the estimates arises—in fact, the residuals from the difference equation will be correlated even if the original observations are independent so that the assumption of independence of residuals in the least squares method used is not valid. It is interesting to note that the problem reduces to one of fitting autoregressive schemes and the constants may be estimated as functions of the autocorrelations.

One would also have liked to see the superior Newton-Raphson technique applied to non-linear least squares equations since they present interesting features over and above ordinary non-linear equations.

With the exception of the above chapter this is a well written and readable book ; the standard of printing is excellent and the layout impressive throughout. G. M. JENKINS.

8.—*Programming for an Automatic Digital Calculator.* By K. H. V. Booth. London, Butterworth, 1958. vii, 238 pp. $8\frac{1}{2}''$. 42*s*.

This manual is comparatively short. It proceeds to introduce the practical mechanics and logic of programming with reference to APEXC (the computer at Birkbeck College). The fact that the exposition is tied to a particular computer is not a handicap because the gain in simplicity of treatment is greater than any loss of comprehensiveness. Every step is carefully explained beginning with the rudiments of "language" and the simplest form of programme and passing on to reiterative processes and more complicated procedures. Chapters are included on subroutines, non-arithmetic programmes (e.g. ordering), matrix

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operations, simultaneous linear equations, mechanical translation, and fault finding and automatic programming. Granted that for the serious student there is no substitute for actually playing with programmes, there is still a need for initial basic reading on the subject. This lucidly written book fills that need excellently. B. BENJAMIN.

9.—*The Investment Decision: An Empirical Study.* By John R. Meyer and Edwin Kuh. Cambridge (Mass.), Harvard University Press, 1957. London, Oxford University Press. xv, 284 pp. 8¼″. 48*s*.

This study was awarded the David A. Wells Prize for the year 1954–55. The work has been very well done and is a good and typical example of present day applied economics. It should be read by those wishing to extend their factual knowledge and those such as Professors Schoeffler and Hogben who are critical of the use of statistical methods and models in economic science.

The authors have made use of various types of knowledge—the results of a multivariate analysis of data from a sample of firms for the years 1946–50, existing theories of investment, general knowledge of the U.S. economy during these years, particular knowledge of individual firms and facts derived from a large number of previous studies. The way in which this accumulation of knowledge has been knitted together is well brought out throughout the book. The statistical methods and their limitations are carefully explained, so carefully in fact that parts of the book provide a good account of correlation and regression analysis in this field of study.

Whilst reading this book the reviewer received a copy of *Economics as a Science* by A. G. Papandreou. It could not have arrived at a better time. Papandreou's book is a methodological study of current practice in economics. He sets out the formal detail of what Meyer and Kuh have attempted to do in practice. Papandreou concludes "On the whole, therefore, economic theorists are engaged in the construction of flexible, general explanatory schemata which may be used in a process of coherent explanation of phenomena associated with individuals, rather than with the construction of a theory which is predictive of behaviour for all historical individuals that share certain attributes. Economics, therefore, has leaned rather heavily toward *ex post facto* explanation in terms of general schemata or models. In these circumstances it is natural to select models on the basis of their (*ex post facto*) explanatory power rather than on the basis of their predictive power".

Meyer and Kuh attempt to explain the behaviour of business men as regards investment in fixed plant and equipment in the American setting of 1946–50—a large-scale industrial economy, oligopolistic markets, the distinct separation of management and ownership, and highly imperfect equity and monetary markets. Their conclusion is that in these circumstances investment outlay in the short run is, on the average, a residual; the difference between the total net flow of funds from current operations and conventional dividend payments. Deviations from this average are related to changes in the value of sales, and the rapidity with which a change in demand is met by an increase in output and capital outlay depends on many factors, the most important being the character of the structure of the market and the extent of the competition for a share of the market.

They have done their job very well, making a valuable addition to our knowledge of the factors influencing economic behaviour in this particular field.

H. W. HAYCOCKS.

10.—*Women's Measurements and Sizes.* Board of Trade, London, H.M.S.O., 1957. xii, 134 pp. 9¾″. 30*s*.

Some 5,000 women between the ages of 18 and 70 submitted to having their body measurements recorded during the early part of 1951. This book describes the techniques and the results of the first scientific study in this country designed to help manufacturers to improve the fit of ready-made women's clothes. Its authors also proffer it in the hope "that it will provide a better understanding of the fundamental proportions of the female figure". It has appeared belatedly under the imprint of the Board of Trade, following the

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dissolution of, firstly, the Clothing Industry Development Council which sponsored the inquiry, and, secondly, the Joint Clothing Council which succeeded to the responsibility. The statistical analysis of the measurements was made by Mr. Kemsley of the Government Social Survey who was also responsible for preparing the report.

A statistician studying this report will do well to leave to the end the opening chapter summarizing the principal results. For it fails to make clear—as does a subsequent chapter—the shortcomings of the sample as a mirror of the female population. It contained too many clerical workers, too many shop assistants, too few professional women, too few housewives, too many Londoners. Where the sample has been rated up to give a guide to the whole population adjustments have been made for age differences between the sample and the population but not for any other variants.

These shortcomings might seem to vitiate the whole exercise. But it is evident that variations from individual to individual are so great that averages are of little help. The survey has concerned itself primarily with finding the relationship between the measurements at different points of a woman's body and recommending a practical sizing system. Doubtless manufacturers would have liked more reliable information on geographical and social variations and the report is deficient in these respects. But even if precise distributions of sizes were possible for the whole population or particular sections of it, these would still not show the buying frequency of different sizes. What the manufacturer does learn from this inquiry is the extent of the variability for which he should be catering.

The report contains a full account, supported by some far-from-glamorous photographs, of how 37 measurements were made on each woman using an extraordinary assortment of equipment which included neck chains, hair nets, and a screwdriver. The 37 frequency distributions are tabulated in full and their arithmetic mean and standard deviation calculated. Correlation coefficients are given between each pair of measurements and multiple correlation coefficients between combinations of key items. Alas, no simple key measurement emerges from this thorough investigation to ease the problem of manufacturers: the recommended size chart is based on the three traditional measurements of height, hip girth and bust girth. By 46 combinations of these measurements it is reckoned that 80 per cent of women would be well served. At present it would seem only about two out of every five women are likely to be fitted without alteration from the usual selection. Perhaps nothing underlines the complexity of the problem so much as the statement that there are no sizes which apply exclusively to one age group and no other.

A mathematical appendix includes an interesting treatment of the bivariate distribution of hip girth and bust girth.

MARJORIE DEANE.

11.—*A Study of the Major Psychoses in an English County.* By Michael Shepherd, M.A., D.M., D.P.M. London, Chapman & Hall, for the Institute of Psychiatry, 1957. 159 pp. $9\frac{3}{4}''$. 25*s*. (Maudsley Monograph, 3.)

This monograph is an account of a follow-up study of the records of patients admitted to the county mental hospital of Buckinghamshire in a pre-war as compared with a post-war triennium. In his introduction the author points out that such simple questions as "What are the characteristics of patients resident in mental hospitals? How long have they been there? What are the differential rates of discharge of patients by sex, age at admission, diagnosis and therapies?", which have been asked repeatedly for more than a century, are still not fully answered. Though the answers suggested in this monograph relate only to patients admitted during two relatively short periods, and to one county hospital, they give some interesting comparisons of post-war with pre-war figures.

The opening chapter describes and discusses in some detail the materials and methods used for the study. The difficulties of making firm and precise diagnoses of mental illness and of establishing sound categories of outcome are emphasized and, because of these difficulties, the author has taken three very broad diagnostic categories and has restricted his criteria of outcome to those administratively measurable by duration in hospital, re-

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admission in the follow-up period and the like. The results are then presented in a chapter giving comparisons between pre-war and post-war figures for the sexes, age-groups, diagnostic categories, first admissions and re-admissions, those discharged and those who died or remained continuously in hospital, and various other sub-groups.

In his final chapter the author forms a number of conclusions. Some of these are warranted by his findings. The main post-war changes were that the patients admitted were older, more were women, more were voluntary patients and, according to the criteria used, they fared better in the five years after admission, i.e. more were discharged, fewer died in hospital, less time was spent in hospital. This improvement in outcome applied especially to the women and to those with affective disorders. These findings are in line with national trends and also with those in many other countries. However, the author's discussion of the possible factors accounting for these trends—for example, that post-war patients might have been admitted at an earlier stage of illness, or that in other ways a changing type of patient was entering hospital, or that national administration or local changes in admission policy were producing these trends—is speculative and could well have been written without this study of hospital admissions having been made.

Certain criticisms of this monograph can be advanced on general, not statistical, grounds. Firstly, the book gives the appearance of being rather more pretentious than was the follow-up study when originally undertaken. The introduction outlines the development of social medicine and emphasizes the fact that knowledge regarding associations with social and economic factors is even scarcer in the field of mental illness than in social medicine generally. But this study does nothing to remedy this lack of knowledge, for the so-called "social" factors examined were those of age, sex and marital status. Indeed, this type of study (a retrospective examination of the records of patients admitted to a hospital whose records, though detailed and consistent, were not concerned with social factors) can hardly answer the socio-medical questions which the author rightly highlights as so urgently requiring answers. The style of writing might also be criticized as too florid for a monograph intended to present numerical data. Words like "subsumption" and phrases like "the exiguous data contained in cognate publications" may be acceptable in themselves, but when occurring continuously they make reading heavy and tend to obscure the figures and rates being presented.

The monograph does not aim at being a statistical study but rather a presentation of numerical descriptions and comparisons of categories and rates. In the appendix the basic data are presented in lengthy tables, and in the text there are many derived tables summarizing the data being compared. It might be suggested that, for the reader interested in psychiatry, there is a preponderance of tables and numerical material which could have been more clearly presented by diagrams. (It might also be mentioned that the basic tables in the appendix and the derived tables in the text do not always agree in totals or in numbers in sub-groups.) The appendix contains a lengthy set of so-called histograms which illustrate, not distributions, but individual time spent in and out of hospital by each patient in each triennium. These histograms, which do not appear to have been used at all by the author, are rather confusing and would have been better omitted.

In spite of these criticisms the monograph contains interesting and useful material. Certain well-known national trends in mental hospital admissions—in particular, the steady rise in the number of admissions, the increasing proportion of women, the growing numbers of elderly and senile patients—are confirmed and analysed in the Buckinghamshire hospital trends. It is of great value to see which age-groups and which diagnostic groups are mainly responsible for these trends and to learn what happened to the various categories of patients after discharge. The author's opinion that the improved outcome for patients admitted in the post-war period was not related to improvements in treatment is also of interest. The problem of mental illness is becoming increasingly recognized by the general public as well as by the psychiatric profession and any investigation which throws light on any aspect of this problem is to be welcomed.

L. STEIN.

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12.—*Cancer Statistics for England and Wales*, 1901–1955, *a Summary of Data Relating to Mortality and Morbidity*. By Alan McKenzie. Studies on Medical and Population Subjects, No. 13. General Register Office, London, H.M.S.O., 1957 (i.e. 1958). iv, 99 pp. 9½". 5*s*. 6*d*.

The increasing interest in and growing concern about cancer makes this summary of the morbidity and mortality data for the years 1901 to 1955 particularly useful. The tables included in the report have been extended or condensed from recent publications of the General Register Office. Although the basic material is not therefore new, it is well arranged to show the principal epidemiological changes in cancer over the first half of this century. During this period, the trend has varied considerably between the sexes. In men, mortality from cancer of "all sites" rose steadily, whereas, among women, it increased until 1921–30 but by the nineteen-forties it had fallen below the 1901–10 rate and continued to fall. At the beginning of the century mortality from all forms of cancer in women was considerably higher than for men ; during the nineteen-twenties the ratio was reversed and, after the next decade, the rate for women fell while that for men continued to rise.

Less than 1 per cent. of all cancer deaths occur in children under 15 years of age. Only a few sites were involved and it is generally accepted that, in a high proportion of cases, they were present at birth.

The most notable change in mortality since 1936 occurred in cancer of the lung and bronchus where there were large increases in the rates for both sexes at ages over 35 years. In men up to 64 years, the rate in 1950–54 was three times that in 1936–39, and in women nearly double, the ratios at older age-groups being very similar. On the other hand, cancer mortality fell for such sites as skin, jaw, lip, tongue, mouth, tonsil, oesophagus, stomach, intestine, rectum and liver. There was little change in mortality from breast cancer in women.

Considerable geographical differences in mortality were found in most sites, but in cancer of the intestines in both sexes and cancer of the female breast the variation was small. Except for cancer of the larynx and oesophagus in women, the urban mortality was higher than the rural in England and Wales as a whole, the greatest differences being found in cancer of the larynx in men and cancer of the lung and bronchus in both sexes. Wales had a notably high mortality in both sexes from cancer of the oesophagus and stomach and in women from cancer of the larynx. In contrast, the mortality from cancer of the lung and bronchus in men and women and cancer of the larynx in men was low. The Southern region of England enjoyed a low mortality for all sites except cancer of the female breast and the South-Western region for all sites except cancer of the oesophagus and larynx in women. Despite its complexity, it is possible to detect certain patterns in distribution of mortality rates.

Five-year survival rates based on cases registered under the National Cancer Scheme during the years 1945–46 are included for the principal sites. Crude rates and corrected rates (corrected for age and the probability of dying during the five-year period from causes other than cancer) are given for four clinical groups: early primary with no secondaries or metastases treated radically, early primary with secondaries or metastases, late primary with or without secondaries or metastases, and all registered cases whether treated or not. In calculating the survival rates, cases lost sight of during the five-year period were counted as dead, thus the rates are probably somewhat below the true figure. Survival rates were generally higher among men for cancer of the lip and stomach and among women for cancer of the tongue, mouth, rectum and bone.

A useful appendix gives a series of mortality tables originally compiled for use in investigating the risks of contracting cancer from employment in specific industries. These tables indicate by sex and age the mortality rate for a number of sites by five-year age groups and for five-year periods since 1911 according to the description of the primary site and the method of assignment of causes of death on death certificates that was in force at the time. While serving their original purpose, the data provide an opportunity for studying cancer mortality trends by cohort analysis.

A. BARR.

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13.—*A Survey of Social Conditions in England and Wales as Illustrated by Statistics.* By A. M. Carr-Saunders, D. Caradog Jones and C. A. Moser. Oxford, Clarendon Press, 1958. xxi, 302 pp. $8\frac{3}{4}''$. 25*s*.

The Social Structure of England and Wales by the first two authors of the present work was first published by 1927; a revised edition was published in 1937. Both of these editions provided very valuable and much used statistical surveys of social conditions in England and Wales at the time of their publication. With the many changes in social conditions since 1937 and with the vast increase in the statistical data relating to them, even the second edition has inevitably lost much of its value as a survey of current conditions. The present authors have produced a completely rewritten version of the book with the title changed to one which is rather more accurately descriptive of the contents. In doing so they have maintained the high standard of the previous editions and have collected together an impressive quantity of statistical information in a commendably readable book.

The general scope of the book remains broadly the same as before with a large part, nearly half, devoted to subjects associated with population or population censuses—although information from many other sources is included. There follow chapters on protective associations, personal income, expenditure, and property, social security, nutrition, health, leisure, religion, and crime. Some of these chapters deal with topics which were not discussed or only discussed briefly in the earlier volumes. The chapter on inborn qualities and most of the discussion of poverty as found by social surveys have now been omitted.

It is a reflection of the increase in the amount of available statistical data that the number of tables has grown from 82 in the first edition and 86 in the second to 143 in the present book. This same increase in data makes more necessary than ever the interpretive guide with which the authors have provided us. While many would have their own different choices of what should be included and what excluded in a survey of social conditions, few would be able to quarrel seriously with the choice which has been made. The result is a balanced account of what may reasonably be claimed to be the most important aspects, at least of those which could be discussed statistically.

In a book of this nature there is much to be said for the view that detailed discussions of definitions and warnings of the limitations and inaccuracies of the data are out of place. For a work written by authors who are clearly aware of these detailed points, the book is remarkably free of them. The resulting gain in intelligibility and readability does, of course, carry its own dangers of giving rise to misleading impressions. There are, however, only a very few places in which the serious student with some understanding of figures (and few others are likely to read a book with as many figures as this) is liable to be misled. One example is the table which gives for the 1951 Census of Population the percentage of married women gainfully occupied without a warning that some of the figures might be serious underestimates of the percentage actually undertaking paid work.

A second example is seen in the following quotation : "Television is not the only form of amusement that has been attracting more customers. Expenditure on private motoring and cycling has increased by about four times between 1946 and 1956." This statement, and its accompanying table using figures from the National Income Blue Book, follows the discussion of quantity data on attendances at cinemas and at football and cricket matches and on the estimated television public; the reader might easily miss the inclusion of price changes in the motor-car comparison. More important, perhaps, is the fact that he might forget that the 1946 figure was severely depressed by the shortage of new cars and the rationing of petrol. He might well be surprised to learn that, according to the same source, expenditure at 1948 prices was only about 30 per cent. higher in 1956 than it was in 1938. In addition, of course, because of the admitted arbitrariness of the division between personal consumers and business, the figures are among the less reliable of those in the Blue Book.

In the discussion of criminal statistics, warnings are given of the pitfalls but the danger might well have been stressed when discussing the figures of the percentage of offences known to the police which are cleared up. It is stated that for fraud the offender is nearly always known but that for others, such as breaking and entering, "detection is difficult and

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not more than 30 per cent. of the cases may be cleared up." Will the reader realize that the successful fraud is almost certainly not known to the police while the successful breaking and entering almost certainly is?

These few points are, however, comparatively trivial and do not have any serious effect on the picture which the student would obtain on reading the book. The authors can hardly be blamed for the possibly serious mistakes which may be made by those lazy enough to try to use the book as a reference book; a clear warning is given that it is not intended for this purpose. The book benefits greatly by the addition of an index in place of the detailed table of contents of the previous edition.

The authors describe their task as pedestrian. So, in a sense, is any work which aims at presenting a selection of the results obtained by others ; it can, nevertheless, be difficult and it has here been carried out with great skill. The present edition should at least maintain the position held by the earlier ones.

W. J. CORLETT.

14.—*The Economics of International Migration.* Edited by Brinley Thomas. London and New York, Macmillan, 1958. xiii, 502 pp. 8¾″ 45*s*.

This book contains the papers and proceedings of the Seventh Conference of the International Economic Association held at Kitzbuhel in September, 1955.

Migration is a topic which is not as well understood as it sometimes appears to be. This conference was devoted entirely to a study of migration in an attempt to produce a complete theory of the causes and consequences of migration.

The immense trans-Atlantic migrations of the nineteenth century appear now to have been a major cause of American development and subsequent prosperity and the question sometimes lurks in the dark parts of the mind "Why can't the present under-developed countries solve their problems in the same way?" In his concluding paper Howard S. Ellis performs a public service by dragging this submerged question out from the depths and showing thereby how easy it is to answer. He gives due weight to exchange control as a factor inhibiting the free transfer of money along with the migrants, to the great differences between population density, climate and natural resources in the United States in 1840 and in the under-developd countries of to-day. The point is also made in discussion that the average real income and expectation of life in "The West" even in the eighteenth century were already substantially higher than in the present under-developed countries and therefore provided a better springboard for further and more rapid development. There is an additional fact, which was not emphasized, that most of the people who migrated to America during the nineteenth century were determined (or resigned) to become citizens of a new country. Few potential or actual immigrants to under-developed countries feel similarly to-day. T. H. Silcock, in a paper dealing with the Far East, considers in some detail the "foreign expert" who is a special sort of migrant, and one of the typical figures of our age. What little migration to under-developed countries there is consists largely of foreign experts who go abroad with every intention of living in, or setting up a little colony of "home beyond the seas". As a result they buy as little as possible from local sources and as much as possible imported from "home", and so minimize their dynamic demand contribution to the economy in which they are living.

There are a number of papers describing the migration experience of individual countries or areas. They provide a useful background against which to assess the theoretical papers and discussions but they would have been even more useful if there had been a bit more uniformity in the nature and style of the statistics presented. The statistical problems of definition and collection were, however, excluded from the scope of the conference so perhaps it is unfair to complain about the statistics actually presented.

The three papers at the beginning of the book do not really achieve a satisfactory analysis. Thomas and Lerner provide brief and stimulating descriptions of the history of international investment and the theory of inflation, and drag migration into the picture by the scruff of its neck. (It must be said that its reluctant presence in no way spoils the party.) Spengler starts by describing and classifying actual migration movements and

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goes on from there but does not reach a comprehensive theoretical formulation. To fill the gaps the reader must study the Summary Record of the Debate at the end of the book. It is a pity that none of the main speakers "subsequently replied in writing" in the manner of Royal Statistical Society speakers whose papers have been carved about at Ordinary meetings of the Society. The discussion was certainly very useful to the participants in the Conference but a digested account would probably provide the complete theory of migration that is not achieved in the book as it stands. A. J. BOREHAM.

15.—*The Control of Inflation.* By J. E. Meade. Cambridge University Press, 1958. 52 pp. $7\frac{1}{4}''$. 5*s*.

This is a reprint of the inaugural lecture delivered by Professor James Meade on taking up the Chair of Political Economy at Cambridge. He poses first of all the question as to whether the annual price rise of 4 or 5 per cent. constitutes a degree of inflation which it is important to control. Professor Meade argues that it does; Britain's trade and financial relations with the rest of the world make even that degree of instability in the currency highly undesirable. Nevertheless he contends that there are worse evils than inflation: control of it should not be at the expense of a high expanding production.

If it is decided that inflation must really be stopped, Professor Meade suggests that the government should commit itself "to the prevention of some precisely defined index of prices from rising above some precisely defined level". The price index proposed would cover all home-produced goods and services except those subject to price control, excluding any import content and indirect taxes. As instruments to regulate quickly the level of demand, he suggests either that the weekly insurance contributions be based on a proportion of earnings which could then be varied, or that quarterly supplements be added to or subtracted from the standard rate of income-tax.

Professor Meade points out the dangers of any such policy unless wage-fixing methods are reformed. He would like to see a body, such as the Cohen Council, estimate the average rise in wages in a coming year that would be compatible with stable prices. It should then be left to an arbitration court to decide whether the wage rise in a particular industry should be above or below the average to avoid a serious shortage or redundancy of labour.

Enough has been said to show that this lecture was bristling with ideas.

MARJORIE DEANE.

16.—*Economic Analysis and Policy in Underdeveloped Countries.* By P. T. Bauer. Durham N.C. Duke University Press and London, Cambridge University Press, for the Duke University Commonwealth—Studies Center, 1957. xiii, 145 pp. 8″. 22*s*. 6*d*.

This little book is principally composed of three lectures which Mr. P. T. Bauer, the Smuts Reader in Commonwealth Studies in the University of Cambridge, gave at the Commonwealth-Studies Center of Duke University in North Carolina in 1956–57. The second and third lectures contain largely a selection from the Cambridge Economic Handbook, *The Economics of Underdeveloped Countries* (London, 1957) written jointly by Mr. P. T. Bauer and B. S. Yamey. The first lecture is a methodological exercise concerning "The Economic Study of Underdeveloped Countries". It argues persuasively that the threefold distinction between positive and normative economics and the art of political economy, always necessary, is especially important in the study of underdeveloped countries. He fears that economics as a systematic discipline may well disintegrate or be radically transformed, unless that distinction is meticulously observed in the proliferating literature on underdeveloped countries. Mr. Bauer's preoccupation is with positive economics in the sense of establishment of functional relations between "economic" variables and the prediction of the probable consequences of specified "economic" occurrences, conditions or circumstances. He warns that a functional relationship which holds under *ceteris paribus* conditions must not be confused with an historical sequence of events, especially not under conditions of rapid changes as in developing countries. Similarly

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prediction of probable consequences must be distinguished from the forecasting of future events in historically unique circumstances.

He pleads for more direct observation in the economic study of underdeveloped countries, i.e., "gathering of information personally or reliance on comparatively unprocessed material, such as files of business firms and government departments, locally published reports, proceedings of representative assemblies, newspapers, advertisements, market reports, auction notices, and the like". He considers direct observations so vital because contrived experiments are impossible in economics, because economic statistics, especially in underdeveloped countries, is often unreliable and because quantification of significant influences in a total social situation is often impossible.

Mr. Bauer would apparently not subscribe to the view that the theory of economic development is dynamic institutional economics. He belongs to the school of economists which holds that economic propositions are timeless and not invalidated by differences in institutions and systems of values in different communities. He confines himself to the analysis of the interrelationships of economic variables. If one of these interacts systematically with political, social, moral, legal or other non-economic variables (which he usually calls just "political") he presumably calls it an historical and not a functional relationship and leaves its examination to others, especially to the "politicians". This attitude is defensible in a culturally stable and economically advanced society where the non-economic variables interacting with economic variables do not, as a rule, change rapidly or unexpectedly. But in a rapidly developing underdeveloped society this attitude will cause the purely economic analysis to appear lifeless and barren. Surely, no economic change in underdeveloped societies is possible without some partly preceding, partly simultaneous and partly succeeding cultural changes, nearly all of which are capable of positive analysis. If the cultural change is preceding (e.g. change in land tenure or education) or simultaneous (e.g. urbanization) the positive systematic relationships to be studied are those between economic and non-economic variables. Only the non-systematic relationships (such as unpredictable government interferences) could be declared to be outside the province of the social scientist.

Mr. Bauer objects quite rightly to normative economics (e.g. development planning) or formulation of policies for total social situations, if they pass under the name of positive economic analysis. But he is not giving valuable advice, if he insists on positive purely economic analysis instead of positive analysis from the point of view of all social sciences together.

The kind of positive economic analysis which he gives is not sufficiently inspiring. It is principally confined to static supply and demand analysis. Nevertheless, he scores many valuable points by just applying static supply and demand theory to the many uncritical, normative and policy opinions expressed in treatises on economic development. This is, indeed, the main purpose of his second lecture entitled "Some Features of the Economic Landscape" and especially of his third lecture called "Some Issues of Policy".

However, his positive analysis suffers from four defects. Firstly, it is static. Nowhere does he discuss functional relationships taking the form of systematic sequences through time. All dynamic changes over time are treated as exogenous and chiefly non-economic changes. Secondly his analysis is partial in the Marshallian tradition. He confines himself to a few variables at a time and forgets the rest. For instance, his Marshallian demand and supply analysis operates with three variables: quantity demanded, quantity supplied and price. This self-imposed limitation is fatal in discussing economic development which is an intensely interrelated process in many directions at the same time. A balanced (equilibrated) growth of factors adopting an economically balanced combination of backward or advanced techniques and their balanced (equilibrated) utilization in many competing and complementary directions when tastes change rapidly is the problem to be studied. If decision-making is not centralized, this situation is equivalent to an increase of the range of effective alternatives open to people. This subject matter requires a general dynamic equilibrium or at least multisector analysis. Thirdly, his analysis does not stress the cumulative and feedback (circular) character of development. A slight stimulus

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might have an avalanche effect. Nothing succeeds like success. Nowhere in his book is the reader made aware of this most important aspect of economic development. Fourthly, during rapid economic development with many unavoidable cultural changes the private and public benefit, short-term and long-term considerations diverge much more than in steadily advancing developed countries. This fact should stand out in any treatise on economic development. But the reader of Bauer's book is not made systematically aware of it.

The reader cannot escape the impression that Mr. Bauer is deeply suspicious of any and every organized economic activity as liable to be inhuman and ruthless and necessarily narrowing the range of individual choice. To be sure, a *laissez-faire* and *laissez-passer* point of view in the theory of economic development is nowadays a refreshing corrective to widespread doctrinaire interventionist attitudes of many authors.

Mr. Bauer, is, of course, aware that forcibly accelerated development is desired in every underdeveloped country and that this forced acceleration is possible only by organized endeavour. His words would be more heeded, if he could bring himself to advocate the preferable choices among the alternative courses of organized action open to such countries. The preferable choices in his own view, to which many economists will gladly subscribe, are those that increase the range of effective alternatives open to people, i.e., which give them more economic freedom of manoeuvre. Some organized actions are for instance of such kind that they take fewer freedoms away for a short time than they create new ones for a long time. The construction of a road between an isolated village and a market by corvée work or out of special taxation (compulsory saving) is an instance. A mere negative attitude to accelerated development by organized effort will cause people to ignore Mr. Bauer's writings—to be sure to their own detriment—but also to his own personal frustration.

He also forgets another burning desire of underdeveloped countries, i.e. the longing for economic equality with the rest of the world. He does not mention the arguments of other authors, e.g. Gunnar Myrdal, that under *laissez-faire* conditions international trade, while benefiting the countries trading, paradoxically increases the relative poverty of underdeveloped countries. This thesis is fervently believed by the nationals of underdeveloped countries and must be met squarely. A mere denial of the deterioration of terms of trade against primary producing and in favour of manufacturing countries is not enough.

All through the book the reader cannot help feeling that the concept of an underdeveloped country which Mr. Bauer uses is not clarified. He enumerates the following characteristics of underdevelopment in the introductory statement: "Low real income and capital per head, large subsistence sector relative to exchange sector and technological backwardness". He gives further characteristics in his longer study written jointly with Mr. Yamey. They, too, do not satisfy. What is apparently needed is a distinction between countries which are undeveloped, undevelopable, underdeveloped and developed with reference to the internally grown and the externally implanted prerequisites for economic development. If these home-grown and implanted prerequisites are not given, a country is temporarily undevelopable. If they are present, but their full utilization is blocked in one way or another, a country is underdeveloped with respect to its own capability for development. If they are given and freely utilized, a country is developed, even if its real income and capital per head are still low and its production techniques are backward relative to other countries. An undeveloped country is still primitive and can be undevelopable or, if the utilization of its capability for development is blocked, underdeveloped. Countries with comparatively high income and capital per head can be currently either undevelopable (stagnating), or underdeveloped or developed.

I have confined my review to some general features of the book and refrained from detailed criticisms because the author had to undertake an agonizing selection of material for presentation in just three lectures. It would be unfair to suggest that he might have left out some matters and put in others. In his third lecture on "Some Issues of Policy" he selects a dozen topics which are certainly well chosen; it would, however, be easy to

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find another twelve topics which would be equally well chosen. Those which he chose are stimulating. And stimulus to thought is all that he could be expected to provide in three lectures.

The lectures were skilfully adapted to Mr. Bauer's audience provided by the Commonwealth-Studies Center of Duke University. The non-economists—historians, political scientists, lawyers, geographers etc.—presumably predominated in numbers in the audience and they undoubtedly benefited greatly by listening to the lectures and Mr. Bauer deserved their grateful applause. All non-economists, the novices among economists and the general reader will find this little book helpful, especially so, if they have previously read uncritical, pseudo-economic books on economic development. JOHN, MARS.

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STATISTICAL NOTES

(1) British Official Statistics

The index of retail prices compiled by the Ministry of Labour and National Service which was 109 in May (prices at January 17th, 1956 = 100) rose to 110 in June and fell to 109 in July and to 108 in August. These are the figures as published to the nearest whole number. Calculated to one place of decimals the figures were 109·2, 110·2, 108·5 and 108·3. The rise in June was due mainly to increases in the average prices of potatoes and meat. In July there were reductions in the prices of potatoes and other vegetables and meat, partly offset by an increase in the price of eggs. The detailed figures of the weights used in calculating the index and the indices for different commodity groups were as follows:

	Food	*Alcoholic Drinks*	*Tobacco*	*Housing*	*Fuel and Light*	*Durable Household Goods*	*Clothing and Footwear*	*Transport Vehicles*	*Miscellaneous*	*Services*	*All Items*
Weights	350	71	80	87	55	60	106	68	59	58	1,000
May 13th, 1958	108·5	105·6	107·8	121·3	109·9	100·2	103·1	112·9	112·7	114·4	109·2
June 17th, 1958	110·9	105·6	107·8	122·2	110·5	100·2	103·1	113·3	112·8	114·5	110·2
July 15th, 1958	106·0	105·8	107·8	122·6	110·7	100·1	102·8	113·2	113·4	114·6	108·5
August 12th, 1958	105·2	105·8	107·8	123·3	110·7	100·1	102·7	113·1	113·3	114·9	108·3

The Ministry of Labour index of weekly wage rates, calculated on the basis of January 31st, 1956 = 100, showed that in May the level was 112·9. It rose to 113·4 in June, 113·7 in July and 114·0 in August. In manufacturing industries only the figures were 112·9, 113·1, 113·3 and 113·5. The principal classes of work-people whose rates of wages rose in the period included building and civil engineering workers, railway workers, general printers, road haulage workers, Local Authority employees, laundry workers, bakers and employees in the main retail distributive trades.

The six-monthly enquiry into average earnings showed that in April 1958, the average earnings per week in the industries covered by the enquiries, i.e. the manufacturing industries and some of the principal non-manufacturing industries and services, were 253*s.* 2*d.* for men and 131*s.* 3*d.* for women, 109*s.* 7*d.* for youths and boys and 85*s.* 7*d.* for girls. These figures represent increases since April 1947 of 105, 95, 132 and 113 per cent. For all workers combined the average was 214*s.* 2*d.*, an increase of 107 per cent. This compares with an estimated rise of about 76 per cent. in weekly full-time *rates* of wages in the same industries and services. In manufacturing industries alone the average weekly earnings in April 1958, were 261*s.* 4*d.* for men and 131*s.* 8*d.* for women. The corresponding hourly earnings were 65·9*d.* and 38·3*d.* The average hours worked per week in all the industries and services were 46·2, the lowest for any date since October 1953.

The total working population and the numbers in civil employment in the four months ended July 1958, were as follows:

(*Thousands*)

Date	*Total Working Population*			*Numbers in Civil Employment*		
	Males	*Females*	*Total*	*Males*	*Females*	*Total*
April 1958 . . .	16,184	7,890	24,074	15,289	7,763	23,052
May 1958 . . .	16,198	7,904	24,102	15,302	7,775	23,077
June 1958 . . .	16,169	7,904	24,073	15,297	7,786	23,083
July 1958 . . .	16,174	7,912	24,086	15,297	7,791	23,088

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The number of persons on the unemployment registers of the Employment Exchanges fell by 18,300 in June and by 17,400 in July. In August it rose by 33,800. The total for August represented 2 per cent. of the total number of employees in Great Britain. The percentage in the different Regions ranged from 1·2 in London and the South-East to 2·9 in the North-West, and 3·7 in Wales and Scotland.

The following is the sex analysis of the figures:

Number of Unemployed Persons on the Registers of Employment Exchanges

	Men and Boys	*Women and Girls*	*Total*
May 12th, 1958 . . .	309,416	138,140	447,556
June 16th, 1958 . . .	297,836	131,421	429,257
July 14th, 1958 . . .	287,677	124,161	411,838
August 11th, 1958 . . .	310,907	134,696	445,603

Of the total of 445,603 in August, 93,757 had been unemployed for less than two weeks, 113,584 for two to eight weeks and 195,292 for over eight weeks, while 42,970 were temporarily stopped. In the four weeks ended August 6th 129,521 vacancies were filled by the Employment Exchanges. The number of vacancies unfilled at this date was 203,237.

In the week ended July 26th, 1958, it is estimated that in manufacturing industries 156,500 manual workers were on short time. This figure was 31,000 less than in June and 60,000 less than in May.

The numbers of insured persons absent from work owing to sickness, including self-employed as well as employed, were 851,300 in May, 819,600 in June, 803,100 in July and 789,600 in August. The numbers of employed persons absent owing to industrial injuries were 58,300, 56,800, 60,400 and 56,200.

The preparation of *Guides to Official Sources No.* 4*: Agricultural and Food Statistics* (H.M.S.O., 1958, 4*s.* 6*d.*) was recommended to the Interdepartmental Committee on Social and Economic Research by its Sub-Committee under the chairmanship of Professor R. G. D. Allen. It relates to all the current agriculture and food statistics of the United Kingdom.

The major section, Part IV, consists of a subject index to statistical series, cross-referenced alphabetically. This describes the main content of the regular series currently maintained by Departments, names the publications in which they appear and indicates the frequency of publication. Parts I to III outline the principal sources and give an account of the various official series. Additional relevant information appears in five appendices. An efficient alphabetical index completes the guide.

(2) Other Statistics

The *World Economic Survey* (U.N., 227 pp. 18*s.*) consists this year of two parts. Part I examines, in compliance with the request of the Economic and Social Council, the problem of inflation, and Part II analyses recent events in the world economy with special reference to the depression of 1957. The report discusses first the main factors underlying the recent advance in prices in industrialized countries with private enterprise economies, then the forces determining recent price advances in the primary producing countries with private enterprise economies, and includes a chapter on the problem of inflation in the countries with centrally planned economies. The second part, analysing recent events, follows the same grouping of countries; for the first two groups, an assessment of the economic outlook for 1958 is given based largely on replies by governments to a questionnaire. The introduction to the survey contrasts the sources and chief characteristics of inflation in the

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three groups of countries and relates past experience to current trends. "The outlook for 1958", it states, is "far from reassuring. Many countries had been forced to curtail imports before the end of 1957 and others followed suit in the early months of 1958. Balance of payment difficulties may well stand in the way of the higher domestic production expected in 1958 ... and in a number of countries investment programmes have been pruned and public expenditure reduced."

The Yearbook of National Accounts Statistics, 1957 (New York, United Nations, 1958, price 18*s*.) is the first of a new series prepared by the United Nations Statistical Office. It is similar in scope and presentation to its predecessor, *Statistics of National Income and Expenditure*, Statistical Papers, Series H, No. 10, published in 1957, which it now supersedes. The *Yearbook* presents estimates of the national income and expenditure of seventy countries and territories for each of the years 1950–56. Wherever possible, estimates are given of the allocation of the gross national product by category of expenditure (at both current and constant prices), by industry of origin and by type of income. Other tables show the composition of gross fixed investment (analysed by type of asset, industry and sector), the composition of consumers' expenditure, the finance of capital formation, details of the revenue and expenditure of public authorities and details of international transactions.

The estimates were prepared by the United Nations Statistical Office with the co-operation of many national statistical offices. The estimates for each country are presented in separate chapters and are classified, as far as possible, according to the United Nations' system of national accounts (*A System of National Accounts and Supporting Tables, Studies in Methods, Series F, No.* 2, U.N., New York, 1953). The Statistical Office has made every effort to present the estimates of the various countries in a form designed to facilitate international comparability and any important differences in concept or coverage are described in the notes which accompany the tables. Besides presenting the estimates, the *Yearbook* also gives a useful summary of the concepted framework and the definitions used in the United Nations' system of national accounts.

CURRENT NOTES

The Trustees of the Houblon-Norman Fund, on the recommendation of their Advisory Committee, have made the following awards for 1958–59.

Research Grants.—Mr. E. A. Boehm, Lecturer in Economics, University of New England, Armidale, New South Wales, "The Economic Depression in Australia in the 1890's"; Mr. A. B. Cramp, Research Student, London School of Economics (Renewal), "Operation of Bank Rate, 1822–60"; Mr. K. F. Dixon, Westminster Bank Ltd., "Early Stages of Development of the London Money Market"; Mr. R. M. Fitzmaurice, Midland Bank Ltd., "Banking in Cornwall, 1700–1900"; Professor J. Jewkes, Professor of Economic Organization, University of Oxford (Renewal), "Economics of the Evolution of the Aircraft"; Dr. V. C. Lutz, "Banking in Italy"; Dr. G. J. de C. Mead, Bank of England (Renewal), "Intendants, as part of the Financial and Administrative System in France in the 18th Century"; Mr. R. G. Opie, Lecturer in Economics, London School of Economics, "Banking in Germany"; Dr. Sidney Pollard, Lecturer in Economic History, University of Sheffield, "The Genesis of Modern Management"; Mr. Neil Runcie, Lecturer in Economics, New South Wales University of Technology, "Hire Purchase in the United Kingdom and the British Dominions"; Dr. S. G. Sturmey, Lecturer in Political Economy, University College, London, "British Shipping"; Professor Brinley Thomas, Professor of Economics, University College of South Wales and Monmouthshire, "Home Investment and Foreign Lending in the United Kingdom, 1890–1913"; Mr. G. A. Yewdall, Research Student, University of Leeds (Renewal), "Population Movements in Certain Northern Towns during the Early Part of the Nineteenth Century".

An offer of awards for 1959–60 will be made in due course. Further information may be obtained from the Secretary, Houblon-Norman Fund, c/o Bank of England, London, E.C.2.

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International Review on Actuarial and Statistical Problems of Social Security, No. 1. (Geneva, International Social Security Association, 1958. v, 145 pp. 9½". £1 per annum (half-yearly publication)): The work of actuaries and statisticians engaged on Social Insurance problems is specialized but important. Until now papers on such problems have been dispersed in various journals; this new periodical, the editorial committee of which consists of a number of distinguished specialists from various countries, aims at bringing together work on these problems at international level. Social Insurance schemes vary greatly in scope, size and financial and administrative arrangements, but many problems are common to all schemes and this periodical should be useful and stimulating.

Each issue will contain technical papers and information on international meetings. This first issue includes a short report on the First International Conference of Social Security Actuaries and Statisticians held in November 1956, and on other recent specialist meetings.

There are seven technical articles of which three are in English, the remainder having English summaries. Three of the articles are of exclusively actuarial interest. The statistical papers include one (in German) on sequential tests; for rejecting or accepting the hypothesis that two random samples are derived from the same universe, comparing distribution functions as well as parameters; others are on the estimation of parameters of frequency curves to derive populations at individual ages from data given in age-groups (in Spanish), and on the theory of stationary processes with a view to its application to the study of time series (in French).

The Council of the Statistical Society of New South Wales is to publish a new journal entitled *The Australian Journal of Statistics*. This journal will be issued three times a year, at the end of April, August, and November. It will be the only journal in Australia devoted wholly to statistics.

Material for publication will be selected by an Editorial Committee appointed by the Council of the Society. Each paper offered for publication will be submitted to at least one referee, who will be an acknowledged specialist, either in Australia or overseas, in the field with which the paper is concerned. Contributions will not be restricted to members of the Society. Papers for inclusion in the Journal should be submitted to either Dr. H. S. Konijn, Department of Economics, The University of Sydney, or to Dr. H. O. Lancaster, School of Public Health and Tropical Medicine, The University of Sydney.

The Bibliography of Nonparametric Statistics and Related Topics, *Journal of the American Statistical Association* (1953), **48**, 844–906, is now being revised. Material published up to and including 1959 will be listed and it is hoped to place more emphasis than before on applications. Mr. I. Richard Savage, Statistics Department, University of Minnesota, Minneapolis, will be glad to receive references (particularly to the non-English literature, and technical reports on the theory or applications of non-parametric statistics as well as corrections and additions to the original bibliography.

The first International Congress of the International Federation for Automatic Control will be held in Moscow from June 25th to July 5th, 1960. The Royal Statistical Society is associated with the Federation through the British Conference on Automation and Control, Group B, and Fellows wishing to submit papers should send abstracts by February 1st, 1959, to Mr. W. Bamford (Honorary Secretary, Group B, B.C.A.C.), c/o The Institution of Electrical Engineers, Savoy Place, London, W.C. 2. Further details of the Congress may be obtained from Mr. Bamford.

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STATISTICAL AND ECONOMIC ARTICLES IN RECENT PERIODICALS

UNITED KINGDOM—

Accounting Research—
Vol. 9 (1958), No. 3—Composition of the corporate sector. I. Some company statistics since 1900; *R. Ma.*

Advancement of Science—
Vol. 14 (1958), No. 57—Animal health in relation to Britain's food supplies; *J. N. Ritchie.* Incentives and communications in industry; *S. B. N. Shimmin, L. J. Buck* and *R. Sergean.*

Annals of Human Genetics—
Vol. 22 (1958), Part 4—Genetics of dermal ridges: the relation between total ridge-count and the variability of counts from finger to finger; *S. B. Holt.* The estimation of linkage on an electronic computer; *H. R. Simpson.* Considerations arising from weight and some other variables recorded in the survey of women's measurements (1957); *M. N. Karn.*

British Journal of Sociology—
Vol. 9 (1958), No. 2—Karl Pearson: socialist and Darwinist; *B. Semmel.*

Computer Journal—
Vol. 1 (1958), No. 1—Parallel programming; *S. Gill.* Mathematics in business; *R. G. Dowse* and *H. W. Gearing.* The solution of railway problems on a digital computer. 1; *A. Gilmour.*
Vol. 1 (1958), No. 2—The use of an electronic computer in research statistics: four years' experience; *F. Yates* and *D. H. Rees.* Statistical foundations for business forecasts; *H. W. Gearing.* The principles of sorting; *D. A. Bell.* The solution of railway problems on a digital computer. 2; *A. Gilmour.* A modified congruence method of generating pseudo-random numbers; *W. E. Thomson.*

Economica—
Vol. 25 (1958), No. 99—Uncertainty, probability and potential surprise; *L. Foldes.*

Economic Journal—
Vol. 68 (1958), No. 270—Capital accumulation and the maintenance of full employment; *D. G. Champernowne.* Factor-price relations under free trade; *R. F. Harrod.* Dynamic demand functions: some econometric results; *R. Stone* and *D. A. Rowe.* Bank credit and gold in the United Kingdom; *W. J. Busschau.* Rate of change approaches to forecasting: diffusion indexes and first differences; *S. S. Alexander.* The European common market and British industry; *H. H. Liesner.* A note on the economics of national superannuation; *J. Black.*

Eugenics Review—
Vol. 50 (1958), No. 2—Fertility and mental illness; *A. Lewis.*

Incorporated Statistician—
Vol. 8 (1958), No. 4—In defence of statistics; *F. A. Friday.* "Lies, damned lies . . . "; *F. D. K. Liddell.* The design of experiments. IV. Factorial design; *M. J. Moroney.*

Journal of Agricultural Economics—
Vol. 13 (1958), No. 1—Planting density and the economics of late flowering chrysanthemum production; *A. B. Webster.* Estimating a supply curve; *F. Holme.*

Journal of the Institute of Actuaries—
Vol. 84 (1958), No. 366—The application of elementary linear programming to approximate valuation; *S. Benjamin* and *C. W. Bennett.* Automatic electronic machines: abstract of discussion. State pensions: statement issued by the Joint Committee of the Institute of Actuaries and the Faculty of Actuaries. Variations in the sex-ratio at birth; *P. G. Moore.*

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Operational Research Quarterly—
Vol. 9 (1958), No. 2—Data for decision making; *A. P. M. Purdon.* Case study: passenger flow in subways; *B. D. Hankin* and *R. A. Wright.* Case study: a study of congestion in the melting shop of a steelworks; *J. Banbury* and *R. J. Taylor.* Case study: operational research study of manufacture at a group of factories; *J. H. Hepburn.* Two years of linear programming; *J. K. Wyatt.*

Population Studies—
Vol. 12 (1958), No. 1—The reproduction rate in Latin America: levels, differentials and trends; *T. Lynn Smith.* A fertility analysis of New Zealand marriage cohorts; *E. G. Jacoby.* The distribution of births in human populations; *W. Brass.*

Stock Exchange Journal—
Vol. 4 (1958), No. 1—Stockbroking as a profession: the missing link; *F. A. A. Menzler.*

AUSTRALIA—
Economic Record—
Vol. 34 (1958), No. 67—The shape of the Australian economy, 1861–1900; *N. G. Butlin.* Comparative productivity in manufacturing industry: a case study of Australia and Canada; *A. Maizels.*

Statistical Society of New South Wales Bulletin—
No. 21 (1958)—Fundamentals of simulation and queueing theory; *B. A. Chartres.* Computers in operations research; *J. M. Bennett.* Some applications of UTECOM to operations research; *R. G. Smart* and *M. E. Oates.* Some sample survey techniques; *D. W. Maitland.*

INDIA—
Calcutta Statistical Association Bulletin—
Vol. 8 (1958), No. 29—University teaching of statistics. On distribution of tolerances in probit analysis with two factors; *S. K. Chatterjee.* A note on the distribution of the successive sums of samples from an exponential population; *D. H. Bhate.* Selection of a subset superior, inferior or equivalent to a control population; *K. C. Seal.* Measurement of attitudes for U-shaped distribution; *P. K. Bose* and *P. K. Sen.* On the large sample behaviour of a multiple-decision procedure; *P. K. Bhattacharyya.*

UNION OF SOUTH AFRICA—
South African Journal of Economics—
Vol. 26 (1958), No. 2—The structure of the cotton textile industry in South Africa; *C. Bak.* Some aspects of South Africa's foreign trade in relation to her aggregate income, 1910–54; *I. R. Woods.*

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American Statistician—
Vol. 12 (1958), No. 3—Organization of statistical observation in the U.S.S.R.; *V. Holubnychy.* Computational formulas for t^2; *P. S. Dwyer.*

Annals of Mathematical Statistics—
Vol. 29 (1958), No. 2—Information theory for mathematicians; *J. Wolfowitz.* Some problems connected with statistical inference; *D. R. Cox.* Asymptotic distribution of stochastic approximation procedures; *J. Sacks.* Admissibility for estimation with quadratic loss; *S. Karlin.* On the estimation of parameters restricted by inequalities; *H. D. Brunk.* Intersection region confidence procedures with an application to the location of the maximum in quadratic regression; *D. L. Wallace.* Exact probabilities and asymptotic relationships for some statistics from *m*-th order Markov chains; *L. A. Goodman.* Tests of multiple independence and the associated confidence bounds; *S. N. Roy* and *R. E. Bargmann.* Sequential tests for variance ratios and components of variance; *A. Birnbaum.* Sums of powers of independent random variables; *J. M. Shapiro.* The staircase design: theory; *F. A. Graybill* and *W. E. Pruitt.* A note on the fundamental identity of sequential analysis; *R. R. Bahadur.* On the statistical treatment of stochastic processes; *I. Fleischer* and *A. Kooharian.* Some remarks on sampling with replacement; *D. Raj* and *S. H. Khamis.* A

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distribution-free upper confidence bound for Pr $\{Y < X\}$, based on independent samples of X and Y; *Z. W. Birnbaum* and *R. C. McCarty*. On the integrodifferential equation of Takács. I; *E. Reich*. Probabilities of hypotheses and information-statistics in sampling from exponential-class populations; *M. Kupperman*. On the distribution of 2×2 random normal determinants; *W. L. Nicholson*. A smooth invertibility theorem; *J. W. Tukey*. On the inversion of the sample covariance matrix in a stationary autoregressive process; *M. M. Siddiqui*. A problem of Berkson, and minimum variance orderly estimators; *J. W. Tukey*. An elementary theorem concerning stationary ergodic processes; *L. Breiman*. A test of fit for multivariate distributions; *L. Weiss*. Tables for obtaining non-parametric tolerance limits; *P. N. Somerville*. Non-parametric estimation of sample percentage point standard deviation; *J. E. Walsh*. A uniqueness property not enjoyed by the normal distribution; *G. P. Steck*. Estimation of a regression line with both variables subject to error under an unusual identification condition; *H. Rubin*. On the decomposition of certain χ^2 variables; *R. V. Hogg* and *A. T. Craig*. A note on the generation of random normal deviates; *G. E. P. Box* and *M. E. Muller*.

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Vol. 26 (1958), No. 2—A probabilistic theory of utility; *R. D. Luce*. An econometric investigation of regional differentials in American agricultural wages; *R. J. Wolfson*. Topology of second order linear difference equations with constant coefficients; *W. J. Baumol*. An experiment in bargaining games; *J. J. Stone*. A note on expectations and stability; *K. J. Arrow* and *M. Nerlove*. A variance formula for marginal productivity estimates using the Cobb-Douglas function; *H. O. Carter* and *H. O. Hartley*.

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Vol. 36 (1958), No. 4—Industrial dynamics: a major breakthrough for decision makers; *J. W. Forrester*. Basic research: should industry do more of it? *A. E. Hickey, Jr.*

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Vol. 14 (1958), No. 12—Psychological bias in attribute sampling; *J. H. Toulouse*. Operating characteristic curves for the lot plot sampling inspection plan when population is normal; *D. H. Shaffer*. A missiles seriousness and complexity vendor rating system; *S. J. Wilson*.

Vol. 15 (1958), No. 1—What is quality control? *C. C. Craig*. Acceptance sampling of lots by the median, quasi-range method. Plotting experimental data on normal or log-normal probability paper; *E. B. Ferrell*. Determination of optimum operating conditions by experimental methods. Part I. Mathematics and statistics fundamental to the fitting of response surfaces; *R. A. Bradley*.

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Vol. 26 (1958), No. 3—A nationwide sample of girls from school lists; *J. W. Bergsten*. The effect of changing the length of an examination on the index of internal consistency; *W. J. Moonan*. Tests of hypotheses about differences between two intercorrelation matrices; *K. W. Schaie*. Test of significance of differences of changes; *C. J. Hoyt* and *P. R. Krishnaiah*. Main effects and non-zero interactions in a two-way classification; *R. O. Collier, Jr.* Significance level in factorial design; *R. McHugh*.

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Vol. 66 (1958), No. 3—How income ought to be distributed: a paradox in distributive ethics; *R. H. Strotz*. Liquidity and farm investment outlays; *N. V. Breckner*. Futures trading and the storage of cotton and wheat; *L. G. Telser*.

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Journal of the American Statistical Association—
Vol. 53 (1958), No. 282—Measuring recessions; *G. H. Moore.* The accuracy and structure of industry expectations in relation to those of individual firms; *R. Ferber.* An index of manufacturing production in New England; *H. B. Ernst.* Effect of varying degrees of transitory income on income elasticity of expenditures; *M. Dunsing* and *M. G. Reid.* The first 1,945 British steamships; *J. R. T. Hughes* and *S. Reiter.* The use of random work sampling for cost analysis and control; *A. C. Rosander, H. E. Guterman* and *A. J. McKeon.* The trentile deviation method of weather forecast evaluation; *M. J. Slonim.* Weight-height standards based on World War II experience; *B. D. Karpinos.* Estimation of survivorship in chronic disease: the "actuarial" method; *L. Elveback.* Graphic computation of *tau* as a coefficient of disarray; *H. D. Griffin.* A new bivariate sign test; *I. Blumen.* Nonparametric estimation from incomplete observations; *E. L. Kaplan* and *P. Meier.* Inadmissible samples and confidence limits; *H. L. Jones.* The precision of unbiased ratio-type estimators; *L. A. Goodman* and *H. O. Hartley.* On noncoverage of sample dwellings; *L. Kish* and *I. Hess.* Complete counterbalancing of immediate sequential effects in a Latin square design; *J. V. Bradley.* Factorial experimentation in Scheffé's analysis of variance for paired comparisons; *O. Dykstra, Jr.* Design and operation of a double-limit variables sampling plan; *A. J. Duncan.* Empiric investigation of a test of homogeneity for populations composed of normal distributions; *G. A. Baker.*

Management Science—
Vol. 4 (1958), No. 4—Some observations on the business school of tomorrow; *G. L. Bach.* Greenhouses of science for management; *H. H. Goode.* Diversification; *W. T. Morris.* A model for diversification; *H. I. Ansoff.* A linear programming model of integrated iron and steel production; *T. Fabian.* Inventory depletion management; *C. Derman* and *M. Klein.* Decision rules for buffer inventories; *C. P. Bonini.*

Monthly Labor Review—
Vol. 81 (1958), No. 7—Manpower measures and the secondary labor force; *I. Sobel.*

Operations Research—
Vol. 6 (1958), No. 3—Simulation techniques in operations research: a review; *J. Harling.* Scale of operations: an empirical study; *E. H. Bowman.* The hub operation scheduling problem; *J. S. Minas* and *L. G. Mitten.* A target-assignment problem; *A. S. Manne.* Application of operations research to development decisions; *B. Klein* and *W. Meckling.* A practical guide to the dual theorem; *H. M. Wagner.* Optimum lot sizes for parts used in aircraft production; *J. C. Chambers, A. F. Bond* and *J. H. Leake.* Two queues under preemptive priority with Poisson arrival and service rates; *F. F. Stephan.* Constructing maximal dynamic flows from static flows; *L. R. Ford, Jr.* and *D. R. Fulkerson.*

Review of Economics and Statistics—
Vol. 40 (1958), No. 2—The renaissance of Soviet statistics; *H. Schwartz.* Communist China's national product in 1952; *A. Eckstein*, with the assistance of *Y. C. Yin* and *H. Yin.* Job analysis and historical productivities in the American cotton textile industry: a study in methodology; *T. Y. Shen.*

Smithsonian Contributions to Astrophysics—
Vol. 2 (1958), No. 9—On two parameters used in the physical theory of meteors; *L. G. Jacchia.*

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Vol. 12 (1958), No. 1—Some relationships between U.S. consumption and natural resources. 1899, 1947, 1954.

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Monatsberichte des Österreichischen Institutes für Wirtschaftsforschung—
Vol. 31 (1958), Beilage 52—Die Schichtung der persönlichen Einkommen in Österreich.

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Annales de Sciences Economiques Appliquées—
Vol. 16 (1958), No. 2—Le transport de passagers sur l'Atlantique Nord; *M. Fontaine.*

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Bulletin de l'Institut de Recherches Economiques et Sociales—
Vol. 24 (1958), No. 4—La Belgique en 1957.

CZECHOSLOVAKIA—
Aplikace Matematiky—
Vol. 3 (1958), No. 3—On the theory of telegraph transmission; *B. Goldschmied* (Czech text, English summary). Ein Nomogramm für die Lösung von Gleichungen vierten Grades; *J. Vurcfeld* (Czech text, German summary).

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Vol. 8 (1958), No. 2—L'influence de l'arrondissement sur les évaluations numériques linéaires; *V. Fabian.* Stochastic branching processes with continuous state space; *M. Jiřina.*

EGYPT—
L'Egypte Contemporaine—
Vol. 49 (1958), No. 292—Les modèles économiques de Walras et de Léontief; *M. Abdel-Malek.*

FRANCE—
Journal de la Société de Statistique de Paris—
Vol. 99 (1958), Nos. 1–2–3—La localisation rationelle des productions agricoles; *J. Klatzmann.* Mortalité et alcoolisation excessive; *S. Ledermann.*

Population—
Vol. 13 (1958), No. 2—Croissance des effectifs scolaires et besoins en maîtres (2e partie); *R. Pressat.* Les travaux de la Commission de l'Equipement scolaire du Commissariat général au Plan; *J. Fourastié.* Evolution de la distance séparant le domicile des futurs époux (Loir-et-Cher 1870–1954, Finistère 1911–1953); *J. Sutter.*

Revue d'Economie Politique—
Vol. 68 (1958), No. 4–5—La France économique en 1957.

GERMANY—
Allgemeines Statistisches Archiv—
Vol. 42 (1958), Heft 1—Untersuchungen über Vereinfachungsverfahren bei Massenaufbereitungen nach quantitativen Merkmalen (erläutert am Beispiel der Ausfuhrstatistik); *S. Koller.* Kohorten-Analyse, insbesondere als Mittel zur Messung der Fruchtbarkeit; *E. G. Jacoby.* Zur Problematik der deutschen Strassenverkehrsunfallstatistik; *G. Schaub.* Zur Frage der Anwendung des geometrischen Mittels, insbesondere bei Berechnung von Indexzahlen; *W. Brachwitz.*

Metrika—
Vol. 1 (1958), Heft 1—Untersuchungen zur Frage der Verteilung von Objekten auf Plätze; *W. Hetz* and *H. Klinger.* Discrete autoregressive schemes with varying time-intervals; *M. H. Quenouille.* Production and inventory planning in a fluctuating market; *S. Danö* and *E. L. Jensen.* Abschätzung der Varianz stetiger Merkmale aus Intervallbreite und Mittelwert; *K.-A. Schäffer* and *M. Nourney.*

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Vol. 80 (1958), Heft 2—Der Zahlungsbilanzausgleich bei beweglichem Zins; *H. Timm.* Zur Frage der westdeutschen Einfuhrelastizitäten; *W. Gatz.*

ITALY—
Annali della Facoltà di Economia e Commercio, Università di Palermo—
Vol. 12 (1958), No. 1—Analisi e nuovi sviluppi dello schema del contagio di Pólya; *C. Ferreri.* Un criterio di scelta di un campione a due strati da un universo iperbolico paretiano; *C. Ferreri.* Aspetti della sensibilità di talune costanti statistiche; *F. Galantino.*

Statistica—
Vol. 18 (1958), No. 2—Di un nuovo prontuario per la risoluzione delle equazioni di terzo grado; *F. Galantino.* Considerazioni sul rapporto dei sessi nei concepimenti; *R. Lenzi.*

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Bibliografie: W. H. Beveridge, F. P. Cantelli, A. Costanzo, A. Del Chiaro, P. de Wolff, F. di Fenizio, G. Fürst, G. Th. Guilbaud, N. Keyfitz and J. Ros-Jimeno.

JAPAN—

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Vol. 9 (1958), No. 3—Discrete decision problems; *Y. Suzuki.* On the estimation of average length of chains in the communication-pattern; *Y. Taga* and *T. Suzuki.* Note on the utilization of the generalized Student ratio in the analysis of variance or dispersion; *M. Siotani.* Note on a characterization of unimodal distributions; *K. Isii.* Efficient small sample nonparametric median tests with bounded significance levels; *J. E. Walsh.* A remark on price analysis in Leontief's open input-output model; *H. Kimura.* Generalized Cauchy distributions; *P. R. Rider.* Modal intervals for chi-square distributions; *M. Sibuya.*

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Statistica Neerlandica—

Vol. 12 (1958), No. 2—The efficiency of some distribution-free tests; *G. E. Noether.* De bloedproef statistisch bezien; *Chr. L. Rümke* and *A. R. Bloemena.* Het examen Statistisch Analyst, 1957 (Algemeen gedeelte). Proeve ener nieuwe personeelsterkte-grafiek; *B. C. Sliggers.* Reconstructie van de spreiding; *J. Muilwijk.*

Vol. 12 (1958), No. 3—Statistische proefopzetten: bewijs en detectie; *J. Hemelrijk.* Statistiek en experiment; *H. C. Hamaker.* Statistiek of experiment; *A. R. van der Burg.* Roteerbare proefopzetten; *A. R. Bloemena.* Het experimenteel bepalen van optimale condities; *G. J. Leppink.* Toepassing van statistiek bij experimenten in een farmacologisch laboratorium; *Chr. L. Rümke.* Statistiek en experiment rondom het poliomyelitis vaccin; *R. van Strik.*

SPAIN—

Trabajos de Estadistica—

Vol. 9 (1958), No. 1—The ranges in small and correlated samples; *P. G. Moore.* Nota sobre la definición de juegos equivalentes; *P. Zoroa.* Aplicaciones de la teoría de juegos; *P. Zoroa.* Problemas de regresión y correlación; *G. Arnaiz.* El Instituto de Estadística Matemática del Japón; *L. Arocas.* Obtención de las curvas de oferta y demanda de bienes agrícolas en España, en los años 1923–934, por el método de los mínimos cuadrados; *A. Martinez Miranda.*

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Vol. 7 (1958), No. 7—Fruktsamheten 1951–1955 samt medelbarnantalet 31/12 1955 i skilda kohorter; *C.-E. Quensel.* Familjebildning och fruktsamhet 1952–1955: en undersökning grundad på uppgifter i befolkningsregistret; *H. Geschwind.*

SWITZERLAND—

Schweizerische Zeitschrift für Volkswirtschaft und Statistik—

Vol. 94 (1958), No. 2—A case study of U.S. monetary policy: reserve requirements and inflationary gold flows in the middle 30's; *K. Brunner.* Critère de sélection pour le choix des variables dans l'analyse de régression; *H. Linhart.* Der Konsument als Bestimmungsfaktor für die Konjunkturentwicklung in den Vereinigten Staaten; *H. Wyss.*

U.S.S.R.—

Teoriya Veroyatnosteĭ i ee Primeneniya—

Vol. 3 (1958), No. 2—Branching stochastic processes for particles diffusing in a restricted domain with absorbing boundaries; *B. A. Sevastyanov* (Russian text, English summary). Ergodic properties of invariant Markov chains on homogeneous spaces; *M. G. Shur* (Russian text, English summary). Strong stability of sums and infinitely divisible laws; *Yu. V. Prokhorov* (Russian text, English summary). A limit theorem for the number of maxima in the sequence of random variables in a Markov chain; *I. I. Gikhman* (Russian

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text, English summary). A statistical problem in detecting a signal in the noise of a multi-channel system reducing to stable distribution laws; *R. L. Dobrushin* (Russian text, English summary). Some problems in the field of limit theorems; *B. A. Rogozin* (Russian text, English summary). Distribution of the super-position of infinitely subdivisible processes; *V. M. Zolotarev* (Russian text, English summary). On characteristic functionals; *V. Sazonov* (Russian text, English summary). Pseudo-random numbers for the machine "Strela"; *I. M. Sobol* (Russian text, English summary).

YUGOSLAVIA—

Statistička Revija—

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Vol. 15 (1957), No. 57—Some recent developments in labour statistics and their implications for the Americas; *R. J. Myers.*

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Vol. 78 (1958), No. 1—Interracial wage structure in certain parts of Africa. Manpower planning in India; *B. N. Datar.*

Vol. 78 (1958), No. 2—Production techniques and employment creation in underdeveloped economies.

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LIST OF ADDITIONS TO THE LIBRARY

Since the issue of Part III, 1958, the Society has received the publications enumerated below.

I.—OFFICIAL PUBLICATIONS

United Kingdom. *Board of Trade. Patent Office.* Periodical publications in the Patent Office Library: list of current titles. 2nd ed. London, H.M.S.O., 1958. v, 258p. 8″. 12*s.* 6*d.*

H.M. Customs and Excise. Classification of chemicals in the Brussels nomenclature ... London, H.M.S.O., 1958. 306p. 8″. 8*s.* 6*d.*

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Medical Research Council. The determination of the ABO and Rh(D) blood groups for transfusion. London, H.M.S.O., 1958. vi, 46p. 9¾″. 3*s.* 6*d.* (M.R.C. Memor. 36, revision of War Memor. 9)

Ministry of Power. Report of the Committee of Inquiry into "coal distribution costs in Great Britain". London, H.M.S.O., 1958. vi, 101p. fold. map. 9¾″. 5*s.* 6*d.* (Cmnd. 446)

Survey of fuel oil used by industry. London, 1957. [i], 14p. 11″.

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Roads in England and Wales: report for the year 1956–57. London, H.M.S.O., 1958. iv, 88p. photos. diagrs. 9¾″. 6*s.* Supersedes *Report on the administration of the road fund.*

Royal Commission on Common Land. Appendix to minutes of evidence. London, H.M.S.O., 1958. 57p. 9¾″. 3*s.*

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Israel. *Central Bureau of Statistics.* Projections of the population of Israel (1955–1970); by Dr. Benjamin Gil. Jerusalem, 1958. Various paging. 10¾″. *I*£2. (Spec. Ser. 69)

Italy. *Istituto Centrale di Statistica.* IX censimento generale della popolazione, 4 novembre, 1951. Vol. VII. Dati generali riassuntivi. Roma, 1958. 455p. 12″. *L*3,500.

Japan. *Bureau of Statistics.* 1955 population census of Japan. Population maps of Japan, 1. Population distribution and density by shi, machi, mura, population census of 1955. Population maps of Japan, 2. Population growth, 1950–1955. Tokyo, 1958. 6 sheets. 43″ × 31¼″.

Kenya. *East African Statistical Department.* Kenya: sample population census of Nairobi, 1957/58; an experiment in sampling methods. [Nairobi], 1958. [ii], 17p. 11″. 2*s.*

Organisation for European Economic Co-operation. A decade of co-operation: achievements and perspectives. Paris, 1958. London, H.M.S.O. 187p. 9½″. 12*s.* (9th Report)

The electricity supply industry in Europe, 1957–1975: 8th enquiry of the Electricity Committee. Paris, 1958. [3]-146p. 9½″. 10*s.* 6*d.* (Trends in Economic Sectors, 4th Year)

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Poland. *Główny Urzad Statystyczny.* Maly rocznik statystyczny, 1958. Rok 1. Warszawa, 1958. x, 182p. fold. map. 5¾″. *Zl.8.*

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REGISTRATION OF THE UNITED KINGDOM AND EIRE

No. I.—ENGLAND AND WALES

BIRTHS, DEATHS *and* MARRIAGES *registered in the Calendar years* 1953–1957 *and in the Quarters of those years. Numbers, Annual and Quarterly Rates*† *per* 1,000 *persons living; (Deaths under* 1 *year of age; rate per* 1,000 *related Live Births. Stillbirths per* 1,000 *births).*

Years	1953		1954		1955		1956		1957‡†	
Estimated Mid-year Home Popn. in thousands*	44,109		44,274		44,441		44,667		44,907	
	Number	Rate	Number	Rate	Number	Rate	Number	Rate	Number	Rate
Live births	681,964	*15·5*	672,635	*15·2*	664,954	*15·0*	698,317	*15·6*	722,952	*16·1*
Stillbirths	15,683	*22·5*	16,202	*23·5*	15,815	*23·2*	16,384	*22·9*	16,554	*22·4*
Deaths	503,529	*11·4*	501,896	*11·3*	518,864	*11·7*	521,331	*11·7*	514,946	*11·5*
Marriages	344,998	*7·8*	341,731	*7·7*	357,918	*8·1*	352,944	*7·9*	346,644	*7·7*
Infant Mortality	18,324	*26·8*	17,160	*25·4*	16,613	*24·9*	16,554	*23·7*	16,641	*23·0*
Net Reproduction Rate	*1·032*		*1·031*		*1·038*		Not yet available		Not yet available	
Quarters	Live Births in the Quarters of each Calendar Year									
Jan.–March	171,649	*15·8*	173,547	*15·9*	169,190	*15·4*	179,151	*16·1*	183,676	*16·6*
April–June	179,477	*16·3*	175,290	*15·9*	171,234	*15·5*	182,400	*16·4*	185,390	*16·6*
July–Sept.	172,943	*15·6*	166,938	*15·0*	166,890	*14·9*	173,534	*15·5*	179,925	*15·9*
Oct.–Dec.	157,895	*14·2*	156,860	*14·1*	157,640	*14·1*	163,232	*14·5*	173,961	*15·4*
	Stillbirths									
Jan.–March	3,975	*22·6*	4,236	*23·8*	4,032	*23·3*	4,311	*23·5*	4,246	*22·6*
April–June	4,220	*23·0*	4,171	*23·2*	4,023	*23·0*	4,248	*22·8*	4,264	*22·5*
July–Sept.	3,711	*21·0*	3,939	*23·1*	3,910	*23·0*	3,948	*22·2*	4,104	*22·3*
Oct.–Dec.	3,777	*23·4*	3,856	*24·0*	3,850	*23·8*	3,877	*23·2*	3,940	*22·1*
	Deaths (excluding Stillbirths)									
Jan.–March	171,350	*15·8*	153,120	*14·0*	168,645	*15·4*	170,292	*15·3*	134,559	*12·2*
April–June	114,644	*10·4*	117,205	*10·6*	124,230	*11·2*	120,059	*10·8*	118,118	*10·6*
July–Sept.	98,545	*8·9*	104,114	*9·3*	101,677	*9·1*	104,131	*9·3*	110,037	*9·7*
Oct.–Dec.	118,990	*10·7*	127,457	*11·4*	124,312	*11·1*	126,849	*11·3*	152,232	*13·4*
	Marriages									
Jan.–March	93,610	*8·6*	95,922	*8·8*	100,696	*9·2*	107,122	*9·7*	110,092	*9·9*
April–June	77,123	*7·0*	74,364	*6·8*	74,763	*6·8*	68,821	*6·2*	65,804	*5·9*
July–Sept.	105,614	*9·5*	104,505	*9·4*	106,691	*9·5*	106,923	*9·5*	103,617	*9·2*
Oct.–Dec.	68,651	*6·2*	66,940	*6·0*	75,768	*6·8*	70,078	*6·3*	67,131	*5·9*
	Infant Mortality									
Jan.–March	5,709	*34·0*	5,256	*31·0*	4,852	*29·2*	4,939	*28·6*	4,438	*25·0*
April–June	4,448	*25·2*	4,304	*24·7*	4,155	*24·4*	4,083	*22·9*	4,031	*22·2*
July–Sept.	3,862	*22·2*	3,543	*21·0*	3,605	*21·6*	3,525	*20·1*	3,853	*21·4*
Oct.–Dec.	4,305	*26·2*	4,057	*25·1*	4,001	*24·5*	4,007	*23·7*	4,319	*24·8*

* Home population comprises the resident civilian population plus any British, Commonwealth or Allied Armed Forces stationed in the country and the mid-year estimates are based upon final figures from the full tabulation of the 1951 Census.

† Birth, death and marriage rates are based on the relevant Home population.

‡ Provisional figures.

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No. II.—SCOTLAND

BIRTHS, DEATHS *and* MARRIAGES *registered in the Calendar years* 1953–1957 *and in the Quarters of those years. Numbers, Annual and Quarterly Rates* per* 1,000 *persons living. (Deaths under* 1 *year of age, rate per* 1,000 *Live Births; Stillbirths per* 1,000 *Births).*

Years	1953		1954		1955		1956		1957†	
Estimated Mid-year Popn. in thousands*	5,118		5,123		5,133		5,145		5,150	
	Number	Rate	Number	Rate	Number	Rate	Number	Rate	Number	Rate
Live Births	90,913	*17·8*	92,315	*18·0*	92,539	*18·0*	95,313	*18·5*	97,978	*19·0*
Stillbirths	2,308	*25*	2,401	*25*	2,330	*25*	2,329	*24*	2,381	*24*
Deaths	58,878	*11·5*	61,380	*12·0*	61,645	*12·0*	61,792	*12·0*	61,143	*11·9*
Marriages	40,902	*8·0*	41,975	*8·2*	43,199	*8·4*	43,971	*8·5*	42,672	*8·3*
Infant Mortality	2,800	*31*	2,861	*31*	2,811	*30*	2,727	*29*	2,802	*29*
Quarters	Live Births in the Quarters of each Calendar Year									
Jan.–March	22,860	*18·1*	23,456	*18·6*	23,111	*18·3*	24,594	*19·2*	24,759	*19·5*
April–June	23,791	*18·6*	24,097	*18·9*	23,942	*18·7*	24,889	*19·5*	24,974	*19·5*
July–Sept.	22,166	*17·2*	22,823	*17·7*	22,734	*17·6*	22,654	*17·5*	24,100	*18·6*
Oct.–Dec.	22,096	*17·1*	21,939	*17·0*	22,752	*17·6*	23,176	*17·9*	24,145	*18·6*
	Stillbirths									
Jan.–March	602	*26*	612	*25*	598	*25*	617	*24*	635	*25*
April–June	586	*24*	628	*25*	566	*23*	613	*24*	602	*24*
July–Sept.	561	*25*	601	*26*	597	*26*	536	*23*	569	*23*
Oct.–Dec.	559	*25*	560	*25*	569	*24*	563	*24*	575	*23*
	Deaths (excluding Stillbirths)									
Jan.–March	17,264	*13·7*	18,003	*14·3*	19,078	*15·1*	19,156	*15·0*	16,175	*12·7*
April–June	14,345	*11·2*	14,638	*11·5*	14,998	*11·7*	14,562	*11·4*	14,427	*11·2*
July–Sept.	12,722	*9·9*	13,327	*10·3*	12,733	*9·8*	13,088	*10·1*	13,537	*10·4*
Oct.–Dec.	14,547	*11·3*	15,412	*11·9*	14,836	*11·5*	14,986	*11·6*	17,004	*13·1*
	Marriages									
Jan.–March	12,085	*9·6*	12,371	*9·8*	12,420	*9·8*	11,744	*9·2*	11,279	*8·9*
April–June	8,212	*6·4*	8,553	*6·7*	8,662	*6·8*	9,845	*7·7*	9,089	*7·1*
July–Sept	12,069	*9·4*	12,490	*9·7*	12,966	*10·0*	12,960	*10·0*	12,976	*10·0*
Oct.–Dec.	8,536	*6·6*	8,561	*6·6*	9,151	*7·1*	9,422	*7·3*	9,328	*7·2*
	Infant Mortality									
Jan.–March	776	*34*	892	*38*	829	*36*	764	*31*	782	*32*
April–June	696	*29*	728	*30*	692	*29*	656	*26*	668	*27*
July–Sept.	614	*28*	555	*24*	588	*26*	602	*27*	653	*27*
Oct.–Dec.	714	*32*	686	*31*	702	*31*	705	*30*	699	*29*

† Provisional figures.

* The population is the civilian population (including members of the Mercantile Marine for the time being in Scotland, accordingly and members of the Armed Forces stationed in Scotland). Birth, Death and Marriage Rates are calculated accord-

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No. III.—NORTHERN IRELAND

BIRTHS, DEATHS *and* MARRIAGES *registered in the Calendar years* 1953–1957 *and in the Quarters of those years. Numbers, Annual and Quarterly Rates* per* 1,000 *persons living. (Deaths under* 1 *year of age, rate per* 1,000 *Live Births).*

Years	1953		1954		1955		1956		1957†	
Estimated Mid-year Home Popn. in thousands*	1,384		1,387		1,394		1,397		1,399	
	Number	Rate	Number	Rate	Number	Rate	Number	Rate	Number	Rate
Live births	28,984	*20·9*	28,803	*20·8*	28,965	*20·8*	29,489	*21·1*	30,108	*21·5*
Stillbirths‡	Not registered									
Deaths	14,813	*10·7*	15,124	*10·9*	15,407	*11·1*	14,858	*10·6*	15,187	*10·9*
Marriages	9,416	*6·8*	9,154	*6·6*	9,510	*6·8*	9,359	*6·7*	9,391	*6·7*
Infant Mortality	1,090	*38*	951	*33*	937	*32*	850	*29*	869	*29*
Quarters	Live Births in the Quarters of each Calendar Year									
Jan.–March	7,212	*21·3*	7,253	*21·3*	6,876	*20·2*	7,216	*20·8*	7,356	*21·4*
April–June	7,902	*22·9*	7,704	*22·3*	7,679	*22·1*	7,912	*22·8*	7,894	*22·6*
July–Sept.	7,254	*20·8*	7,089	*20·3*	7,638	*21·7*	7,379	*21·0*	7,676	*21·8*
Oct.–Dec.	6,616	*19·0*	6,757	*19·3*	6,772	*19·3*	6,982	*19·9*	7,182	*20·4*
	Deaths (excluding Stillbirths)									
Jan.–March	4,542	*13·4*	4,311	*12·6*	5,083	*14·9*	4,880	*14·1*	4,109	*11·9*
April–June	3,733	*10·8*	3,985	*11·5*	3,867	*11·1*	3,485	*10·0*	3,631	*10·4*
July–Sept.	3,075	*8·8*	3,197	*9·1*	3,126	*8·9*	2,972	*8·5*	3,259	*9·2*
Oct.–Dec.	3,463	*9·9*	3,631	*10·4*	3,331	*9·5*	3,521	*10·0*	4,188	*11·9*
	Marriages									
Jan.–March	2,022	*6·0*	2,067	*6·1*	2,185	*6·4*	2,074	*6·0*	2,390	*6·9*
April–June	2,288	*6·6*	2,123	*6·1*	2,180	*6·3*	2,204	*6·4*	2,133	*6·1*
July–Sept.	3,235	*9·2*	3,183	*8·9*	3,297	*9·5*	3,410	*9·7*	3,217	*9·1*
Oct.–Dec.	1,871	*5·4*	1,781	*5·0*	1,848	*5·3*	1,671	*4·8*	1,651	*4·7*
	Infant Mortality									
Jan.–March	332	*46*	285	*39*	268	*39*	257	*36*	255	*35*
April–June	294	*37*	245	*32*	231	*30*	211	*27*	212	*27*
July–Sept.	225	*31*	202	*28*	216	*28*	178	*24*	184	*24*
Oct.–Dec.	239	*36*	219	*32*	222	*33*	204	*29*	218	*30*

* Home population comprises civilians and members of the armed forces stationed in N. Ireland. Birth, death and marriage rates are calculated accordingly.
† Provisional figures.
‡ Stillbirths are not registered in N. Ireland. The birth of one living child and one stillborn, or one living and two stillborn, is counted as one birth.

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No. IV.—EIRE

BIRTHS, DEATHS *and* MARRIAGES *registered in the Calendar years* 1953–1957 *and in the Quarters of those years. Numbers, Annual and Quarterly Rates* per* 1,000 *persons living. (Deaths under* 1 *year of age, rate per* 1,000 *Live Births).*

Years	1953		1954		1955		1956†		1957†	
Estimated Mid-year Popn. in thousands	2,942		2,933		2,909		2,895		2,885	
	Number	Rate	Number	Rate	Number	Rate	Number	Rate	Number	Rate
Live births	62,558	21·2	62,534	21·3	61,622	21·2	60,738	21·0	61,286	21·2
Stillbirths‡					Not registered					
Deaths	34,591	11·7	35,535	12·1	36,761	12·6	33,898	11·7	34,308	11·9
Marriages	15,888	5·4	15,831	5·4	16,443	5·7	16,567	5·7	14,423	5·0
Infant Mortality	2,463	39	2,364	38	2,264	37	2,161	36	2,026	33
Quarters	Live Births in the Quarters of each Calendar Year									
Jan.–March	15,672	21·3	15,379	21·0	15,183	20·9	14,021	19·3	14,738	20·4
April–June	16,947	23·0	16,487	22·5	16,480	22·7	16,840	23·3	16,453	22·8
July–Sept.	15,728	21·4	15,974	21·8	15,807	21·7	15,219	21·0	15,414	21·4
Oct.–Dec.	14,211	19·3	14,694	20·0	14,152	19·5	14,658	20·3	14,681	20·4
	Deaths (excluding Stillbirths)									
Jan.–March	10,544	14·3	10,470	14·3	11,808	16·2	10,370	14·3	9,699	13·4
April–June	8,948	12·2	8,847	12·1	9,731	13·4	8,595	11·9	8,200	11·4
July–Sept.	7,104	9·6	7,673	10·5	7,362	10·1	7,056	9·7	7,302	10·1
Oct.–Dec.	7,995	10·9	8,545	11·7	7,860	10·8	7,877	10·9	9,107	12·6
	Marriages									
Jan.–March	2,935	4·0	3,332	4·5	3,135	4·3	2,813	3·9	2,601	3·6
April–June	3,816	5·2	3,642	5·0	3,989	5·5	4,130	5·7	3,278	4·5
July–Sept.	5,533	7·5	5,507	7·5	5,792	8·0	5,616	7·8	5,430	7·5
Oct.–Dec.	3,604	4·9	3,350	4·6	3,527	4·8	4,008	5·5	3,114	4·3
	Infant Mortality									
Jan.–March	703	45	749	49	677	45	632	45	567	38
April–June	686	43	594	36	599	36	557	33	520	32
July–Sept.	517	33	473	30	471	30	455	30	460	30
Oct.–Dec.	557	39	548	37	517	37	517	35	479	33

* Rates are based on the total estimated population as at the middle of the corresponding year.
† Provisional figures.
‡ Stillbirths are not registered in Eire. The births of one living and one stillborn, or of one living child and two stillborn are counted as one birth.

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REVENUE OF THE UNITED KINGDOM

Net Product in Quarters of 1957, *and the Financial Years ended March* 31*st*, 1957–58, 1956–57, 1955–56, 1954–55

£ *thousand*

	Quarters ended				*Total for Calendar Year* 1957
	March 31*st*, 1957	*June* 30*th*, 1957	*Sept.* 30*th*, 1957	*Dec.* 31*st*, 1957	
Ordinary Revenue—					
Income tax	1,313,171	298,375	319,483	249,215	2,180,244
Surtax	104,700	24,500	12,200	16,400	157,800
Death duties	48,300	45,900	44,800	35,500	174,500
Stamps	17,100	16,400	18,000	15,700	67,200
Profits tax and excess profits tax	45,850	40,700	79,100	75,400	242,200
Excess profits levy	1,150				
Other Inland Revenue duties	430	50	60	40	580
Total Inland Revenue	1,530,701	425,925	473,643	392,255	2,822,524
Customs	289,626	299,602	303,589	315,510	1,208,327
Excise	226,400	206,360	239,840	257,925	930,525
Total Customs and Excise	516,026	505,962	543,429	573,435	2,138,852
Motor vehicles duties	64,193	11,555	9,749	8,847	94,344
Post Office (net receipt)	1,214	5,600	—	7,100	13,914
Broadcast receiving licences	8,800	5,600	5,400	10,200	30,000
Receipts from sundry loans	2,399	8,868	13,999	4,763	30,029
Miscellaneous receipts	65,756	50,197	41,884	30,773	188,610
Total Ordinary Revenue	2,189,089	1,013,707	1,088,104	1,027,373	5,318,273
Self-balancing Revenue—					
Post Office	82,286	71,900	81,400	82,700	318,286
Income tax deducted from excess profits tax, post-war refunds	87	37	156	77	357

£ *thousand*

	Year 1957–58	*Year* 1956–57	1957–58 (*compared with* 1956–57)		*Corresponding Years*	
			Increase	*Decrease*	1955–56	1954–55
Ordinary Revenue—						
Income tax	2,208,266	2,114,173	94,093	—	1,942,902	1,892,965
Surtax	157,400	158,000	—	600	138,600	134,700
Death duties	170,600	169,000	1,600	—	175,700	187,900
Stamps	63,700	63,000	700	—	70,600	75,100
Profits tax and excess profits tax	255,150	195,000	55,150	—	192,500	172,800
Excess profits levy		5,000			18,450	76,500
Other Inland Revenue duties	550	640	—	90	770	1,280
Total Inland Revenue	2,855,666	2,704,813	150,853	—	2,539,522	2,541,245
Customs	1,207,452	1,198,882	8,570	—	1,148,598	1,100,049
Excise	942,360	901,735	40,625	—	864,511	771,637
Total Customs and Excise	2,149,812	2,100,617	49,195	—	2,013,109	1,871,686
Motor vehicle duties	100,734	90,610	10,124	—	86,987	78,980
Post Office (net receipt)	7,785	4,464	3,321	—	—	5,100
Broadcast receiving licences	30,700	28,400	2,300	—	25,800	22,300
Receipts from sundry loans	32,167	32,714	—	547	29,858	23,023
Miscellaneous receipts	165,720	196,195	—	30,475	197,867	195,558
Total Ordinary Revenue	5,342,584	5,157,813	184,771	—	4,893,143	4,737,892
Self-balancing Revenue—						
Post Office	335,515	304,036	31,479	—	264,900	243,600
Income tax deducted from excess profits tax, post-war refunds	575	599	—	24	2,365	5,186

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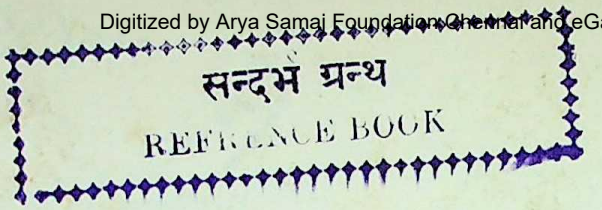

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